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# The Effect of Non-Native Pupils on Natives' Learning: Evidence from the EU

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# Contents

## Abstract

This report studies the effect of increasing the number of students with a migrant background in a classroom on native students' test scores in reading and mathematics. It uses data on primary school children from two large international surveys that allow to produce estimates for most EU member states. The report does not find strong evidence for either negative or positive spillover effects of students with a migrant background on native students. The lack of any sizeable effect is independent of the starting level of concentration of students with a migrant background in the classroom: for both classes with few and classes with, relatively, many students with a migrant background, the estimated effect of increasing the number of students with a migrant background is close to null. The report also explores whether an urban/rural gap exists in the ability of schools to integrate students with a migrant background finding no difference.

# 1 Introduction

Migration to Europe has been on the rise in recent years. The increasing trend has affected both countries with a long history of immigration, often related to a colonial past, and countries with historically low migration inflows. As the majority of immigrants are of working age, policy makers and practitioners alike have focused primarily on the impact of these flows on labour market outcomes and frequently the labour market outcomes of natives only. This attention has sparked a very fertile research agenda (Borjas, 2003; D'Amuri and Peri, 2014; Dustmann et al., 2013; Ottaviano and Peri, 2012) that has, so far, failed to reach a consensus[1]. However, as these cohorts of migrants age and settle in their destination countries, the consequences of their presence starts to reverberate through all spheres of host countries societies. Educational systems are one of these spheres and deserve special attention for at least two reasons: the pivotal role that they play in the formation of human capital and the consequent repercussion for private and societal wellbeing; the recognition that integration of migrants necessarily runs through the integration of their children that develops primarily within schools.

One of the most accepted regularities within the field of economics is that schooling is essential in determining future economic success (Becker, 1975; Card, 1999; Griliches, 1977). It is then clear that how students with a migrant background fare in school is essential both for the obvious implication on their own future wellbeing and for the wellbeing of their native classmates. As the share of students with a migrant background in schools is destined to increase in the near future, more and more natives will be studying in classes where some, or at times many, of their peers will be of foreign background. Understanding the potential repercussion of a more diverse group of students on learning outcomes is therefore paramount.

Even though these concerns have only marginally attracted the attention of migration practitioners, they are undeniably front and center both in the minds of parents - driving their residential location decisions – and school officials - affecting their educational practices. Whether these preoccupations are justified is hard to asses. An argument often made is that large concentration of disruptive students, intended as students who need more attention than the average, is harmful to their peers. This argument is theoretically justified (Lazear, 2001) and could apply to students with a migrant background for two reasons: a) they usually come from families of poor socio-economic background; b) their language proficiency is often lower than that of natives requiring increased teacher attention which is subtracted to that dedicated to the rest of the class.

Empirically, the argument is less clear. On the one hand, we have some supporting evidence for how peer group composition matters for students' performance (Lavy et al., 2012; Markman et al., 2003). On the other, evidence on specific immigrant peer effects on natives is still scarce, possibly due to the relative novelty of the phenomenon[2], and far from conclusive. This article aims at providing additional evidence on this matter from a cross-European perspective.

The analysis will focus on students with a migrant background spillover effects[3] on primary school children between 8 and 10 years old. This differentiates the analysis from most of the literature on immigrant spillover that mainly considers secondary schools and almost never for more than one country at a time. Focusing on younger students offers two types of advantages. First, it allows evaluating how students respond to an early exposure to a diverse peer group. Second, growing evidence (Almond and Currie, 2011; Heckman, 2008; Heckman et al., 2013; Knudsen et al., 2006) establishes not only that early childhood environments substantially affect later life outcomes, but also their effect on learning is larger than those triggered in later stages. Studying peer effects at early stages of the curricula allows us to concentrate on what is arguably the key phase of both hard and soft skills formation.

If the evidence on students with a migrant background peer effects (especially in the European context) is scarce, even scarcer is evidence from harmonized cross-country studies that allows for meaningful comparisons between educational systems[4]. Even if affected by widely accepted limitations, cross-country studies can be

[1] It needs to be mentioned though that the majority of the estimated effects in the literature are small. See Longhi et al. (2005) for a summary and meta-analysis of the literature on the wage effect of immigrants.

[2] A comprehensive literature review on the topic is provided in Section 2.

[3] Consistently with the economic literature on this topic, the term 'spillover effect' is used here and in the remainder of this report to indicate the effect that the presence of studemts with a migrant background has on the learning outcome of their native classmates.

[4] The only other paper that I am aware of adopting a cross-country perspective is Brunello and Rocco (2013) that will be discussed in the next section.

useful to shed light on how different institutional settings are able to facilitate students' integration. This report is able to produce harmonized cross-country evidence by exploiting two different datasets covering most Member States (MS)[5]: the 2016 Progress in International Reading Literacy and Study (PIRLS) and the 2015 Trends in International Mathematics and Science Study (TIMSS). The first dataset provides data on reading skills while the second focuses on math and science.

Methodologically, the identification of peer effects is complicated by self-selection. If students with a migrant background were randomly allocated to schools, we could obtain meaningful estimates of spillover effects by simply comparing the outcomes of native students exposed to many students with a migrant background to those of native students in less diverse classes. Unfortunately, families with a migrant background are likely to settle in areas with more immigrants. As mentioned before, native parents fearing, rightfully or not, that schools in these areas might not offer the best learning environment to their children could opt either to move to other areas or to take their children to other schools. Moreover, the families that are more likely to *opt-out* from immigrant-dominated schools are those who value education more and who are, on average, offering a more conducive learning environment to their kids. This implies that the native children who are left in students with a migrant background-dominated schools will be disproportionally drawn from the less able, or the worst performing, ones.

To overcome the issue of selectivity, I follow Ammermueller and Pischke (2009) and Ohinata and van Ours (2013) and identify peer effects controlling for unobserved school characteristics via school fixed-effects (FE). This procedure hinges on random allocation of students *within*, but not *between* schools. That has to say that as long as students are allocated randomly between classes *within* the same school, the estimates produced will reveal the true spillover effect even in the presence of non-random allocation of pupils *between* schools. The assumption of non-random allocation within schools might be too restrictive as some recent literature seems to suggest (Ballatore et al., 2018), but offers the considerable advantage of being applicable in a consistent manner in a cross-country setting as the present one[6]. Furthermore, concerns of ability sorting have been shown to be less relevant for primary school students (Ammermueller and Pischke, 2009) than at later stages of the curricula.

The main findings of this report are that *unconditional* peer effects[7] are negative in most MSs, meaning that increasing the share of children with a migrant background in class negatively influences the test scores of native children. However, the negative effects tend to disappear once differences in individual and school characteristics and self-selection are taken into account as the benchmark results show that immigrant-peer effects are zero or close to zero in most MSs for both reading and math scores. This report also finds some degree of heterogeneity between countries. Nonetheless, even for the most negative case (Finland), the effects are statistically significant, but substantively small.

Additionally, this report examines whether a rural/urban divide exists in the ability to integrate students and whether peer-effects start operating only at higher concentrations of students with a migrant background in class by allowing for non-linear peer-effects, but it finds no support for either conjectures.

[5] A full list of the countries considered in this study is presented in Section 3.

[6] It would not be possible to adopt such strategy with other well-known cross-country data source such as PISA as this survey does not provide information at class level. This is a major advantage of using PIRLS and TIMSS data.

[7] With unconditional peer-effects I refer to the peer effects observed in the raw data, before controlling for students characteristics and school selectivity.

# 2 Previous studies

Educational spillovers between native and immigrant children have received some attention in the economic literature. Most evidence comes from the US and has considered several possible aspects: college enrolment rates (Borjas, 2007; Hoxby, 1998); the probability of high school graduation (Betts, 1998; Hunt, 2016), the number of years of schooling (Betts and Lofstrom, 2000).

In the European context, research is scarcer, but growing[8] and in recent years several studies appeared focusing on Austria (Schneeweis, 2015), Italy (Ballatore et al., 2018; Frattini and Meschi, 2019; Tonello, 2016), The Netherlands (Ohinata and van Ours, 2013), The United Kingdom (Geay et al., 2013) and the Nordic countries (Andersen and Thomsen, 2011; Hardoy and Schøne, 2013; Jensen and Rasmussen, 2011). Very few papers, instead, adopted a trans-national perspective; an exemption worth mentioning is Brunello and Rocco (2013) who analyse PISA data on 27 countries, among which 20 EU countries.
All these studies assess the effect that class segregation has on pupils, often focusing on native pupils only, in terms of some form of test scores. What they often differ on, other than the geographical focus, is the estimations method adopted. This is a crucial aspect if, as it often is the case, the interest of the researcher lies in establishing the causal effect of peer composition on performance in school.

The estimation of a causal effect is complicated by at least three mechanisms. First, families do not locate randomly. Parents, or future parents, often take the quality of the local schools into account when deciding to move in an area. Since migrants tend to concentrate in areas where public infrastructures, and specifically schools, are worse, their native peers will often be the low-achieving ones as parents who care about their children education and can move to better areas will tend to do so. Second, parents have some leeway in choosing their favoured school even without having to relocate, for example by picking among the best available school within their catchment area or by choosing a private institution outside of it. Third, educational authorities can assign pupils and teachers non-randomly. For example, they could assign the best teachers to classes with the highest concentration of migrant students to lift their learning outcomes or, vice-versa, choose to prioritize predominantly native classes to please native parents.

From this brief discussion on the main empirical issues, it should be clear that a researcher who is interested in estimating educational spillovers, cannot rely on naïve regressions of a measure of school outcome on personal and peers' characteristics to establish the effect of class segregation on educational achievements as the parameters so obtained would be skewed and furthermore, the direction of the skew is a-priori undetermined.
The reason for the disparate estimates produced in the literature reflects, at least partially, the potential to account for these unobserved factors of the different research designs adopted. Broadly speaking, the different strategies adopted fall into three categories depending on the type of variation in class concentration that they exploit: quasi-experimental, within school or between-cohorts.

Well-designed quasi-experimental studies have the advantage of accounting convincingly for most, if not all, of the unobserved mechanisms described above. Geay et al. (2013) and Ballatore et al. (2018) are two studies exploiting a so-called natural experiment generating exogenous variation in the share of immigrant pupils in a classroom for estimating educational spillovers.

Geay et al. (2013) look at British students in primary school exploiting the exogenous variation generated by the EU enlargement to Eastern European countries in 2004. Following this enlargement, the share of non-native English speakers in the U.K. soared, especially for catholic schools since many immigrants came from Poland, a predominantly catholic country. Under the assumption that the composition of native students within catholic schools was not affected by the EU enlargement, the authors estimate a causal effect of non-natives on natives students close to 0 or marginally positive for math scores by comparing the outcomes of natives students in catholic schools to that of students in other type of schools before and after the enlargement.

Ballatore et al. (2018), instead, exploit the particular institutional feature of class creation for Italian primary schools to generate the exogenous variation needed for identification. The Italian Ministry of Education's guidelines prescribe a target size of 25 pupils per class. Students pre-enroll in February for the following year starting in September. Based on this pre-enrolment, local school authorities tentatively decide the number of classes for the following school year capping their size to 25. Immigrants are typically less likely to pre-enroll,

[8] See Brunello and De Paola (2017) for a review of the evidence from European countries.

consequently they are allocated to classes only in September when the school year starts and they show up at their local institution. Since law caps the maximum number of pupils per class to 25, these late enrollers might force the creation of additional *splitting* classes. While the old classes are usually predominantly native, the new, usually smaller, *splitting* classes have a high share of migrants. The interaction between the rules of class formation and native enrolment generates an exogenous source of variation for the number of immigrants in a class. Differently from Geay et al. (2013) they find a sizeable negative effect: adding one immigrant to a class reduces native performance in both language and math by approximately 1.6% in 2nd grade and this effect does not fade away in 5th grade.

The works of Andersen and Thomsen (2011), Jensen and Rasmussen (2011) and Tonello (2016) also try to exploit a seemingly exogenous variation, but pure exogeneity in their setting is somehow more questionable. Andersen and Thomsen (2011) use the share of bilingual children enrolled in upper Danish secondary schools in each municipality to predict the concentration of immigrants in primary schools. Jensen and Rasmussen (2011) and Tonello (2016) use the number of residents with a migrant background within the schools catchment areas in Denmark and Italy respectively, as an exogenous source of variation for the share of pupils with a migrant background in schools. All three studies find negative effects, stronger in the case of Denmark than in Italy, of students with a migrant background on natives' test scores, but these strategies are less convincing as they assume that the characteristics of the area of residence have no effect on student performance other than through school segregation.

Instead of using quasi-experimental variation, Ohinata and van Ours (2013) (whose methodology I have applied in this paper) and Frattini and Meschi (2019) rely on variation of the concentration of children with a migrant background between classes within the same school. This methodology goes to great lengths to correct for a large set of unobserved factors. It is still vulnerable though to the possibility that principals allocate students according to their ability, or that teaching resources, in the form of smaller classes for example, are reserved to classes with a higher share of immigrants.

Ohinata and van Ours (2013) look at the impact of pupils with a migrant background on native Dutch primary school students by comparing achievements in math, reading and science between classes with different shares of immigrants within the same school. They are able to control for a large set of variables capturing children, parents and teacher background. Their main result is the substantive absence of any spillover effect.

Frattini and Meschi (2019) use administrative data on vocational high schools in Lombardy, the most populous Italian region, exploiting between classes and between cohorts variation in the presences of immigrant students. They find that the presence of students with a migrant background in the classroom has no effect on natives' literacy scores, but it negatively affects their math scores, but the negative effects are small.

The bias introduced by endogenous allocation of immigrant students can be bypassed by aggregating data at the school level and treating the between cohorts demographic variation in the share of immigrant students as random. Hardoy and Schøne (2013) apply this strategy to Norway looking at the effect on drop-out rates finding sizeable negative effects, while Schneeweis (2015) to Austria tracking grade repetition finding no effects for native students, but large negative ones for immigrant students.

To conclude, Brunello and Rocco (2013) is the only study looking at cross-country effects. They exploit cross-country differences in the share of immigrant students to estimate spillover effects. This aggregation removes across schools sorting of students, but prevents them to account for important observable differences in student backgrounds that might drive the results. They find small, but statistically significant, negative effects of pupils with a migrant background on the test score of native children.

# 3 Data and Descriptive Statistics

As already mentioned, this study adopts a cross-European perspective and estimate spillover effects in most MSs. For this purpose, it resorts to two large international surveys: the 2015 TIMSS for mathematics and science and the 2016 PIRLS for reading scores. The two surveys are very similar. They are both maintained and designed by the International Association for the evaluation of Educational Achievements (IEA). Besides the difference in subject tested, the main differences between the two surveys are that PIRLS collects information for 35 countries, while TIMSS for 40. TIMSS also collects information for eighth graders, but for the sake of comparability, this study concentrates on fourth graders only, whose information is collected in both surveys. Students in both surveys are selected with a two-staged sampling design. First, schools are selected using a probability-proportional-to-size scheme; second, one or more fourth grade classes from the selected schools are randomly sampled.

The two dependent variables in this study are test scores in mathematics and reading. The test designed by IEA measure the ability of fourth graders to solve mathematical questions and read. Given the young age of the test subjects, IAE has been careful in designing a testing procedure purposely aimed at minimizing loss of concentration and fatigue. Testing time is limited to 80 minutes per student, with an additional 15–30 minutes for a student questionnaire. Tests are divided into 12 blocks that are each expected to require 40 minutes of student testing time. Each block is then distributed among individual student booklets. In both tests, scores are standardized to an international mean of 500 and a standard deviation of 100.

The two datasets contain extensive information on pupils' characteristics, demographic characteristics of their parents, their teachers and their schools.

Regarding the students, it includes information on gender, age, whether the language in which they are tested is the language commonly spoken at home and whether they are born in the country of current residence. Parents' information includes their highest level of education and, for TIMSS only, also whether they are born in the country where they currently reside, among others. For teachers, their gender, years of experience, age and their level and field of study is known. School characteristics include class size and the size of the population of the area where the school is located.

In this report, I use the information on children birthplace and parents' birthplace to define the population of interest. In fact, students are defined as having a migrant background if they are born outside of the current country of residence[9]. The benchmark spillover effects are the estimated starting from the computation of the percentage of this type of students in class; these percentages are then included in a multiple regression model and regressed on the learning outcomes of native students in the same class, obtaining my coefficient of interest. In section 4.2 Including students with parents of a migrant background check the robustness of my main finding to a change in the definition of having a migrant background. In that section, also the children born in the current country of residence, but from parents born outside of that country[10] are lumped together with students of a migrant background. I define this enlarged group of non-native students as `students with parents of a migrant background'.

Even if the information collected by the two surveys is very extensive and compares favorably to that collected in most commonly used surveys, two shortcomings need to be noted. First, the datasets do not have information on the exact country of birth of the pupil nor of her parents. This means that it is impossible to distinguish between EU mobile citizens and Third Country Nationals. In this paper, both groups are defined as having a *migrant-background*. It has to be stressed though that in the literature on peer effects it is standard practice to apply the same definition of migrant-background applied here overlooking any distinction between EU and non-EU migrants. In the literature, positive or negative peer effects are mostly linked to linguistic deficiencies of children with a migrant background. Seen under this light it should be clearer why the intra/extra EU distinction is less relevant in this type of literature as linguistic distance matter for both groups. Second, as it is shown in Figure 1, the concentration of students with a migrant background among the classes surveyed rarely exceeds 20% of the total. This means that the results presented here might not hold at very high levels of immigrant children concentration.

[9] This group is often referred to as "first-generation" immigrants.
[10] This group is often referred to as "second-generation" immigrants.

Figure 1 presents the percentages of classes at selected shares of first-generation immigrant pupils differentiating between geographical areas, from cities above 500,000 to villages of less than 3,000 inhabitants using PIRLS data[11]. This figure highlights how the concentration of immigrant students in class is higher in cities than in rural areas. In almost 80% of the classes surveyed in small villages less than 5% of the students were born outside of the country. At the same time, no class in small villages has a concentration of migrant students above 20% while these levels are experienced in more than 5% of classes in big cities. It is also worth noting that small villages are outliers. Other than a slightly higher presence of high concentration classes in big cities, class concentration is very similar once the population of the reference area is above 3,000.

**Figure 1**. First-generation pupils' class concentration by geographic area

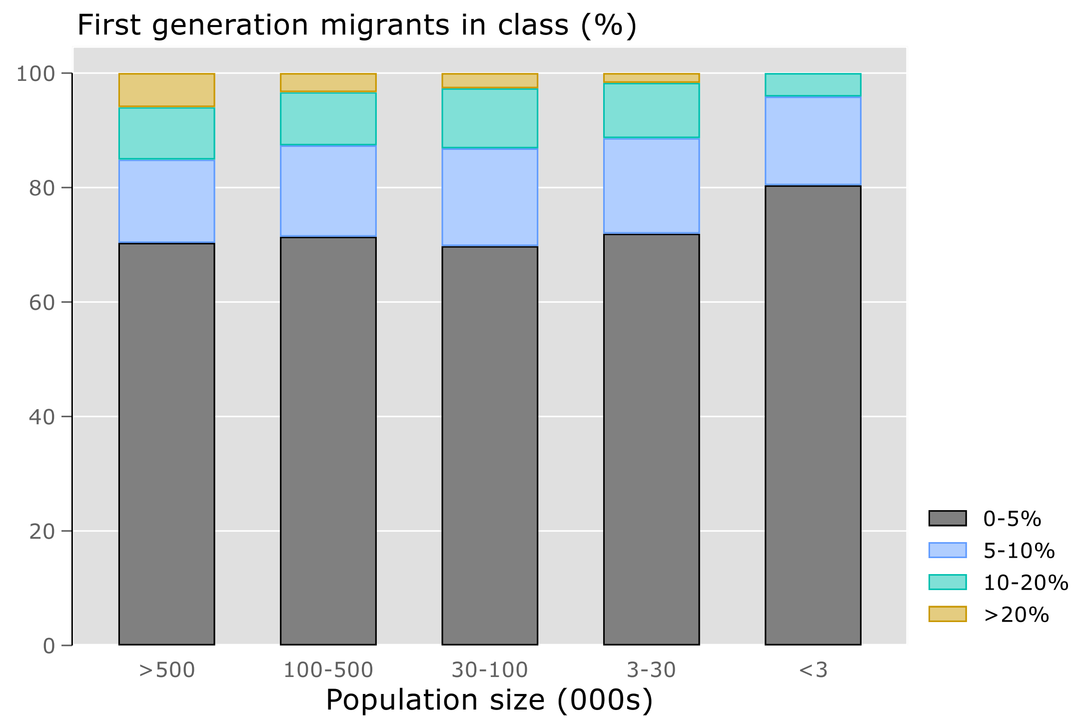



**Table 1**. Descriptive Statistics - PIRLS

| | 0-5% | 5-10% | 10-20% | >20% | Total |
|---|---|---|---|---|---|
| Avg. class reading sc. | 549.0<br>(40.88) | 542.6<br>(41.93) | 532.5<br>(47.22) | 510.8<br>(55.54) | 545.6<br>(42.70) |
| Female | 0.496<br>(0.500) | 0.506<br>(0.500) | 0.510<br>(0.500) | 0.491<br>(0.500) | 0.499<br>(0.500) |

[11] TIMSS data show very similar shares. Results available on request.

| | | | | | |
|---|---|---|---|---|---|
| Age | 10.29 | 10.24 | 10.21 | 10.23 | 10.28 |
| | (0.567) | (0.556) | (0.537) | (0.586) | (0.564) |
| Father's Education: | | | | | |
| Primary or below | 0.031 | 0.036 | 0.048 | 0.102 | 0.035 |
| | (0.173) | (0.186) | (0.214) | (0.302) | (0.184) |
| Secondary | 0.569 | 0.553 | 0.487 | 0.506 | 0.557 |
| | (0.495) | (0.497) | (0.500) | (0.500) | (0.497) |
| Short tertiary | 0.081 | 0.093 | 0.119 | 0.105 | 0.088 |
| | (0.273) | (0.290) | (0.324) | (0.307) | (0.281) |
| Bachelor and above | 0.320 | 0.319 | 0.346 | 0.287 | 0.321 |
| | (0.466) | (0.466) | (0.476) | (0.453) | (0.467) |
| Mother's Education: | | | | | |
| Primary or below | 0.025 | 0.029 | 0.048 | 0.113 | 0.029 |
| | (0.154) | (0.168) | (0.214) | (0.316) | (0.169) |
| Secondary | 0.481 | 0.474 | 0.449 | 0.485 | 0.477 |
| | (0.500) | (0.499) | (0.497) | (0.500) | (0.499) |
| Short tertiary | 0.092 | 0.108 | 0.121 | 0.095 | 0.097 |
| | (0.288) | (0.311) | (0.326) | (0.293) | (0.296) |
| Bachelor and above | 0.403 | 0.389 | 0.382 | 0.308 | 0.397 |
| | (0.490) | (0.488) | (0.486) | (0.462) | (0.489) |
| Female teacher | 0.898 | 0.891 | 0.840 | 0.876 | 0.891 |
| | (0.302) | (0.312) | (0.366) | (0.330) | (0.311) |
| Teacher exp. (yrs.) | 20.17 | 19.20 | 17.30 | 15.88 | 19.65 |
| | (11.07) | (11.76) | (11.01) | (11.56) | (11.24) |
| Teacher age < 30 | 0.105 | 0.123 | 0.167 | 0.194 | 0.115 |
| | (0.306) | (0.328) | (0.373) | (0.395) | (0.320) |
| Teacher age 30-40 | 0.216 | 0.251 | 0.255 | 0.343 | 0.228 |
| | (0.412) | (0.434) | (0.436) | (0.475) | (0.420) |
| Teacher age 40-50 | 0.317 | 0.286 | 0.294 | 0.213 | 0.308 |
| | (0.465) | (0.452) | (0.456) | (0.410) | (0.462) |
| Teacher age > 50 | 0.362 | 0.341 | 0.283 | 0.250 | 0.348 |
| | (0.480) | (0.474) | (0.451) | (0.433) | (0.477) |
| Class size | 23.28 | 21.90 | 22.40 | 22.03 | 22.95 |
| | (4.183) | (4.402) | (4.240) | (4.025) | (4.256) |
| N | 35,368 | 7,803 | 4,393 | 1,162 | 48,726 |

Note: Standard deviations in parentheses. PIRLS data

Table 1 presents summary statistics by the proportion of immigrants in class for the PIRLS sample. These data cover 48,726 fourth-graders from 1,506 distinct schools and 3,023 classes, living in 18 Member States: Austria, Belgium, Bulgaria, Czech Republic, Finland, France, Hungary, Ireland, Italy, Latvia, Lithuania, Malta, Poland, Portugal, Slovakia, Slovenia, Spain and Sweden. PIRLS data cover an additional four member states: Denmark, Germany, the Netherlands and the UK. Denmark and Germany are dropped as all classes sampled there are drafted from different schools. That means that it is not possible to compare their scores to classes within the same school which is the identification strategy adopted in this paper. The Netherlands and the UK, instead, are eliminated because no information on the country of birth of the parents is collected making it impossible to determine whether the pupil is a second-generation immigrant.

From Table 1 it is clear how the majority of students in my sample are in classes where students with a migrant background are less than 5% of the total. It also indicates that the average reading score diminishes as the concentration of students with a migrant background in class increases. Classes where students with a migrant background account for more than 20% of the total have reading scores that are 6% lower than the sample average. Regarding personal pupils characteristics, gender split is fairly even and age is slight above 10 years.

Looking at parent's education, parents of children in classes' with a high concentration of students with a migrant background are less educated than the sample average; this is true for both parents.

As for teachers' characteristics, they are almost exclusively females, more experienced and older in classes with low concentration of students with a migrant background. Class size is, on average, in the low 20s and slightly smaller when the concentration of students with a migrant background is high.

**Table 2.** Average class test score in mathematics - TIMSS

| | 0-5% | 5-10% | 10-20% | >20% | Total |
|---|---|---|---|---|---|
| Avg. class math score | 528.2<br>(41.91) | 521.3<br>(43.41) | 522.7<br>(39.84) | 504.9<br>(43.12) | 526.2<br>(42.22) |
| N | 44,026 | 9,678 | 4,608 | 1,142 | 59,454 |

Note: Standard deviations in parentheses. TIMSS data.

Background characteristics for the TIMSS sample are very similar to those for PIRLS[12]. The main difference between the two samples concerns the countries covered. As stated earlier, after my selection rules, my PIRLS sample covers the 18 member states mentioned earlier; the TIMSS sample, instead covers the following twenty MSs: Belgium, Bulgaria, Czechia, Denmark, Finland, France, Germany, Hungary, Ireland, Italy, Lithuania, The Netherlands, Poland, Portugal, Slovakia, Slovenia, Spain, Sweden and the U.K.. For the same reasons mentioned above, I drop Denmark, Germany, The Netherlands and the U.K.. This leaves me with fifteen MS, some of which differ from those covered by PIRLS.

Since observable characteristics are similar in the two samples, for sake of brevity, in Table 2 I only show the average test score in mathematics by the proportion of immigrants in class. As for reading scores, we can see that test results decrease as the percentage of pupils with a migrant background in class increases.

[12] Results available on request.

# 4 Estimation

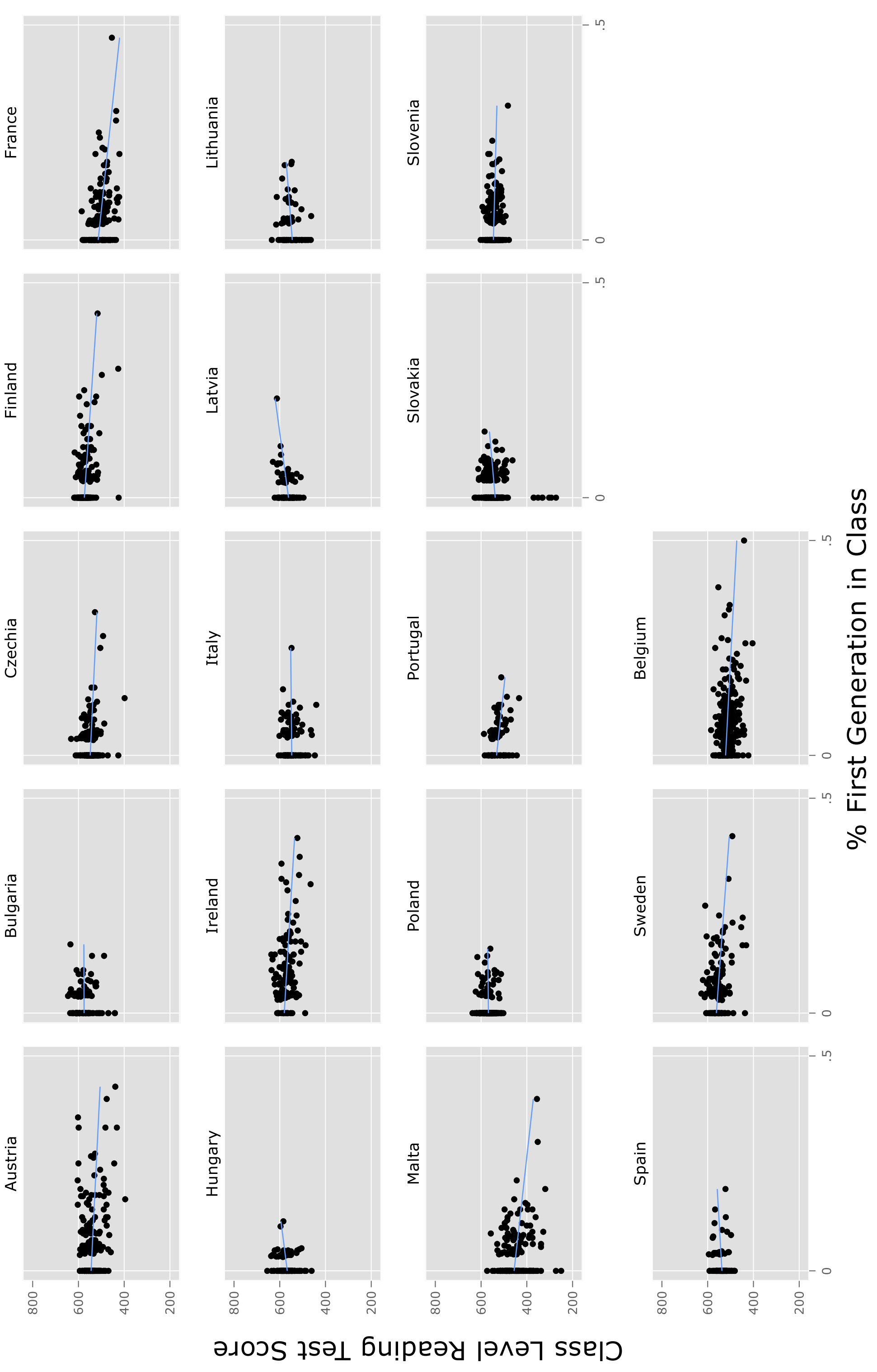
The focus of this paper is the effect of migrant pupils on the learning outcomes of native pupils.

Table 1 and Table 2 show that as the share of students with a migrant background in class increases, class average scores tend to diminish. This same relationship is visually displayed in Figure 2 where, for each country, I plot the percentage of migrants in class and the average class reading test score (Panel a) and the percentage of migrants and the average class math test score (Panel b). A line of best fit is added to the scatterplots to highlight the direction of this relationship. For reading scores in Panel a) the picture shows that, for 10 out of 18 countries this relationship is negative, in five cases positive while there seem to be no clear association in three countries. For mathematics score in Panel b) the picture is similar with 10 countries displaying a negative relationship, four a positive one and no clear relationship in Spain.

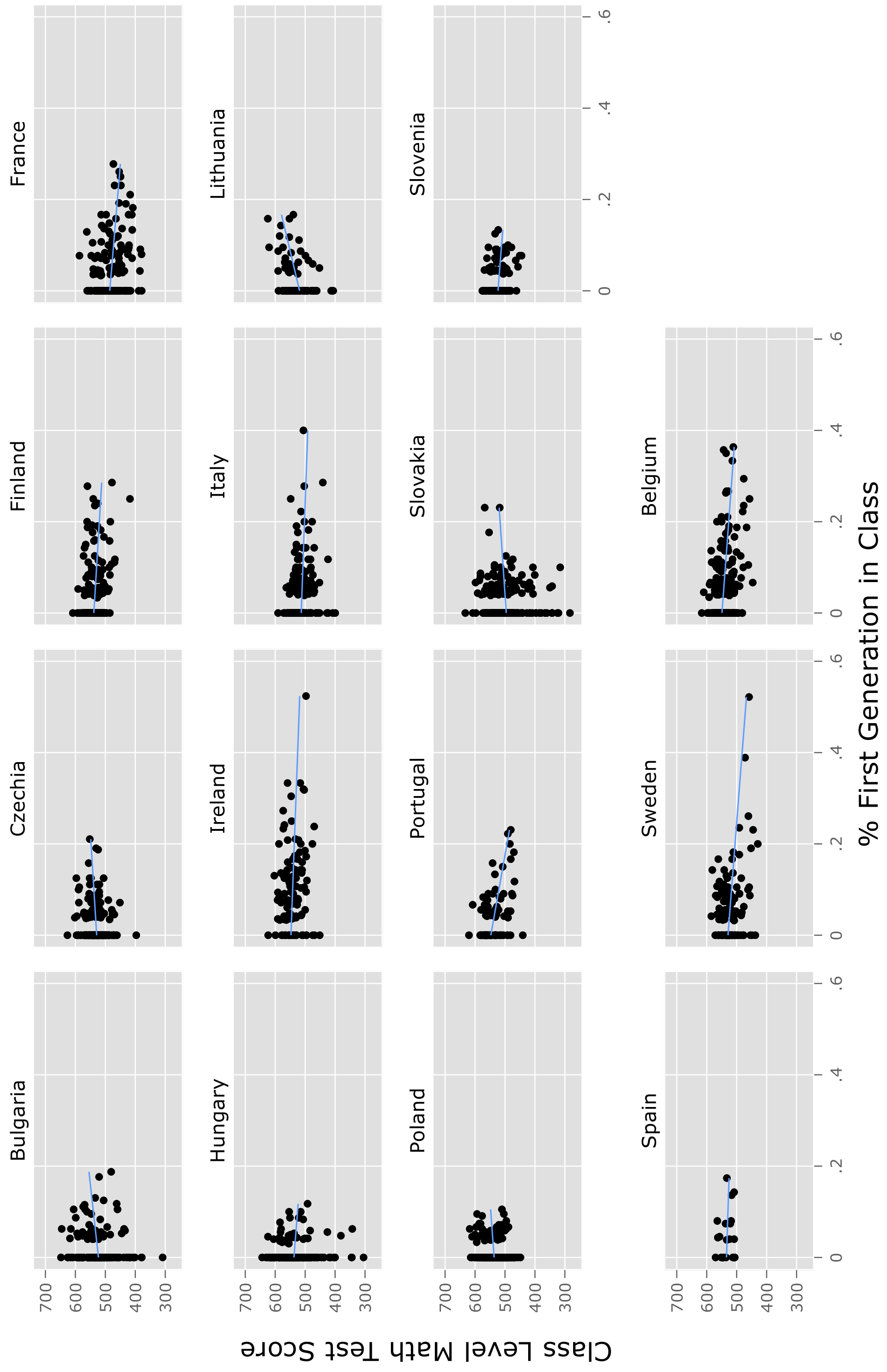
But raw statistics might mask other mechanisms at play. For example, looking again at the descriptive statistics, we can appreciate how classes with many students with a migrant background are usually thought by younger and less experienced teachers and the level of education of parent's in these classes is lower. Both factors will conceivably depress test score independently from pupils' origin. The independent effect of the concentration of students with a migrant background on test scores can more meaningfully be estimated by including these observable characteristics in a multiple regression model and estimate it via ordinary least squares (OLS).

**Figure 2**. Percentage of first-generation pupils in class and average class achievement scores

a) Reading Scores



b) Mathematics Scores




The OLS estimator would produce unbiased estimates of my main effect if the distribution of native pupils were independent of the concentration of students with a migrant background. Unfortunately, there is ample evidence that this is not the case[13]. Parents take into account the composition of their children classes when selecting their future school. This will be especially true for high-income families and highly educated parents. This non-random sorting will cause students with a migrant background to be assigned to classes together with - on average - low achieving natives as the highest achieving natives will have left the pool. This endogenous sorting, sometimes referred to as "white flight" in the literature, will cause the estimated effect of migrant pupils on native pupils to be negatively inflated if left unaccounted for.

To deal with this endogenous sorting I follow Ammermueller and Pischke (2009) and Ohinata and van Ours (2013) and identify the peer effect of interest by exploiting the plausibly random within-school variation in peer characteristics across classes in the same cohort by including school fixed-effects in the regression model. In other words, I will identify the migrant peer effects by comparing the reading and math test scores across classes within the same school that happen to have different shares of students with a migrant background, keeping some important (observable) demographic and school characteristics constant. This strategy will produce reliable estimates if students are allocated randomly within a school. This estimation strategy also requires that only schools for which more than one class is observed can be considered. Therefore, I exclude from my sample all schools who only participate to the survey with one class.

The choice of eliminating all single-classes schools might bias the estimation results if these schools were different from the multiple-classes schools. One way of addressing this concern is to look at existing differences in observable characteristics between the two groups. This is what Table 3 does by comparing students, teachers and schools observable characteristics for schools with one or multiple classes in my PIRLS sample[14]. The first Column shows the sub-sample averages for the latter group while Column 2 those for the earlier one and the last Column presents the results of a t-test for the significance of the difference between the two sub-samples. We can appreciate how most differences are statistically significant, but very small in magnitudes. The only major differences are for teachers experience and age that are both higher in schools where only one class is sampled. Importantly, there are no major differences in tests scores and the concentration of students with a migrant background. These results suggest that the schools dropped are very similar to those kept for the analysis.

[13] Native flight has been documented for different countries. See for example Betts and Fairlie (2003) and Cascio and Lewis (2012) for the US, Geay et al. (2013) for the UK, Rangvid (2010) for Denmark and Karsten (2006) for the Netherlands.

[14] TIMMS data show very similar patterns. Results available on request.

**Table 3.** Mean values of observed characteristics single vs. multiple classes' schools.

| | 2+ Classes<br>(1) | 1 Class<br>(2) | Diff.<br>(3) |
|---|---|---|---|
| Female | 0.498 | 0.504 | -0.006<br>(0.004) |
| (log)reading score | 6.299 | 6.293 | 0.007***<br>(0.001) |
| % First gen. migr. | 3.885 | 3.746 | 0.140**<br>(0.045) |
| Highest edu. father | 2.694 | 2.561 | 0.133***<br>(0.007) |
| Highest edu. mother | 2.861 | 2.761 | 0.099***<br>(0.008) |
| Teacher exp. (yrs.) | 19.714 | 21.139 | -1.425***<br>(0.087) |
| Teacher age (cat) | 2.895 | 3.023 | -0.128***<br>(0.008) |
| Teacher age < 30 | 0.115 | 0.070 | 0.045***<br>(0.002) |
| Teacher age 30-40 | 0.226 | 0.234 | -0.008*<br>(0.003) |
| Teacher age 40-50 | 0.307 | 0.299 | 0.008*<br>(0.004) |
| Teacher age > 50 | 0.351 | 0.397 | -0.046***<br>(0.004) |
| Class size | 22.965 | 22.861 | 0.104**<br>(0.037) |
| Area pop. ('000s) | | | |
| >500 | 0.130 | 0.121 | 0.010***<br>(0.003) |
| 100-500 | 0.155 | 0.164 | -0.009**<br>(0.003) |
| 30-100 | 0.211 | 0.176 | 0.035***<br>(0.003) |
| 3-15 | 0.442 | 0.353 | 0.088***<br>(0.004) |
| <3 | 0.061 | 0.185 | -0.124***<br>(0.002) |

Note: standard errors in parentheses. PIRLS data.

Let us now turn to the estimation of the relationship between the share of students with a migrant background in class and test scores. In reading these results, we should always keep in mind that they are unbiased under the assumption of random allocation of migrant students *within* school[15], but robust to non-random allocation of students *between* schools.

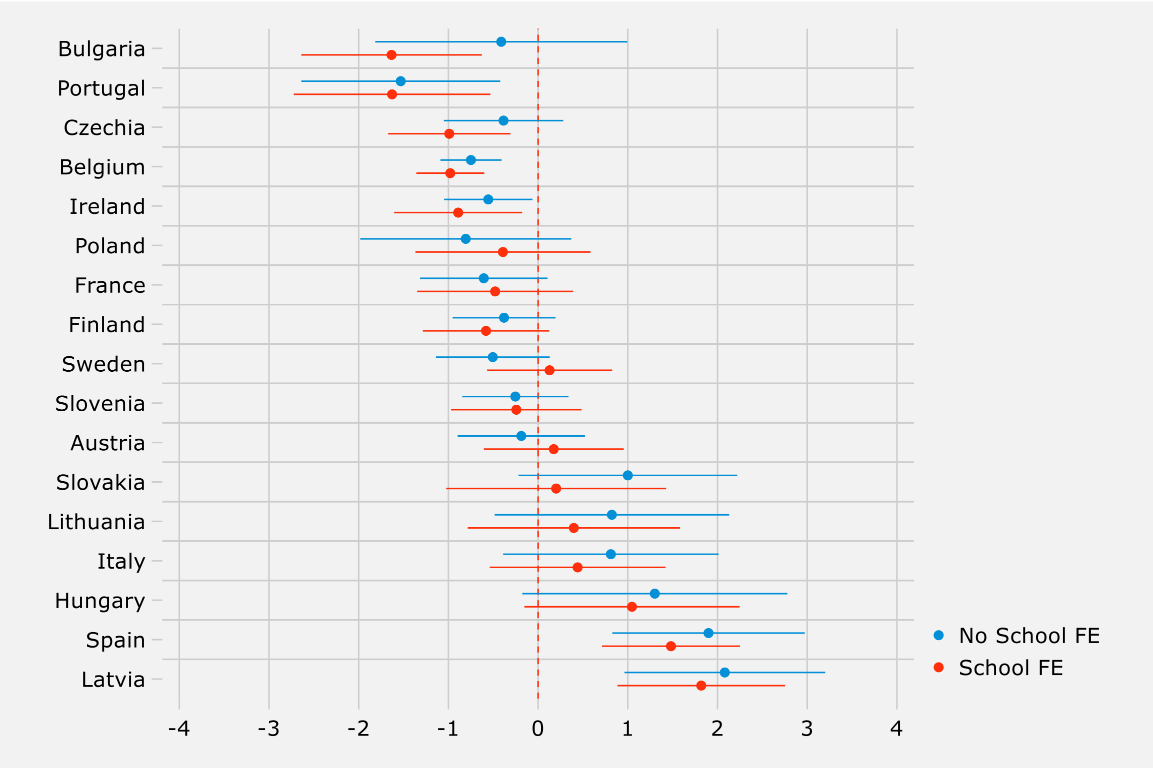
[15] Remember that PIRLS's and TIMSS's sampling design ensures that classes are randomly selected within schools.

## 4.1 Baseline estimates

Figure 3 Panels a) and b) show the estimated spillover effect for immigrant on native pupils for reading and mathematics scores respectively. The Figure shows two estimates for each Member State considered, one obtained with an OLS estimator, the other with a fixed-effect (FE) model, for the key variable of interest, namely the effect of increasing the share of immigrant pupils in class on the two learning outcomes. All regressions also include a common set of control variables: a quadratic term in student age, student gender, parent's educational achievement, teachers' gender (interacted with pupil's gender), age and years of experience and class size[16]. The dependent variables in these regressions are native students reading and mathematics scores that are a standardized measure with international mean of 500 and standard deviation of 100. For ease of interpretation, a vertical line at zero is added to the graph and a line representing the confidence interval is added to each dot representing the point estimate. If the confidence interval crosses the zero, the estimated parameter is not statistically significant at the 5% level.

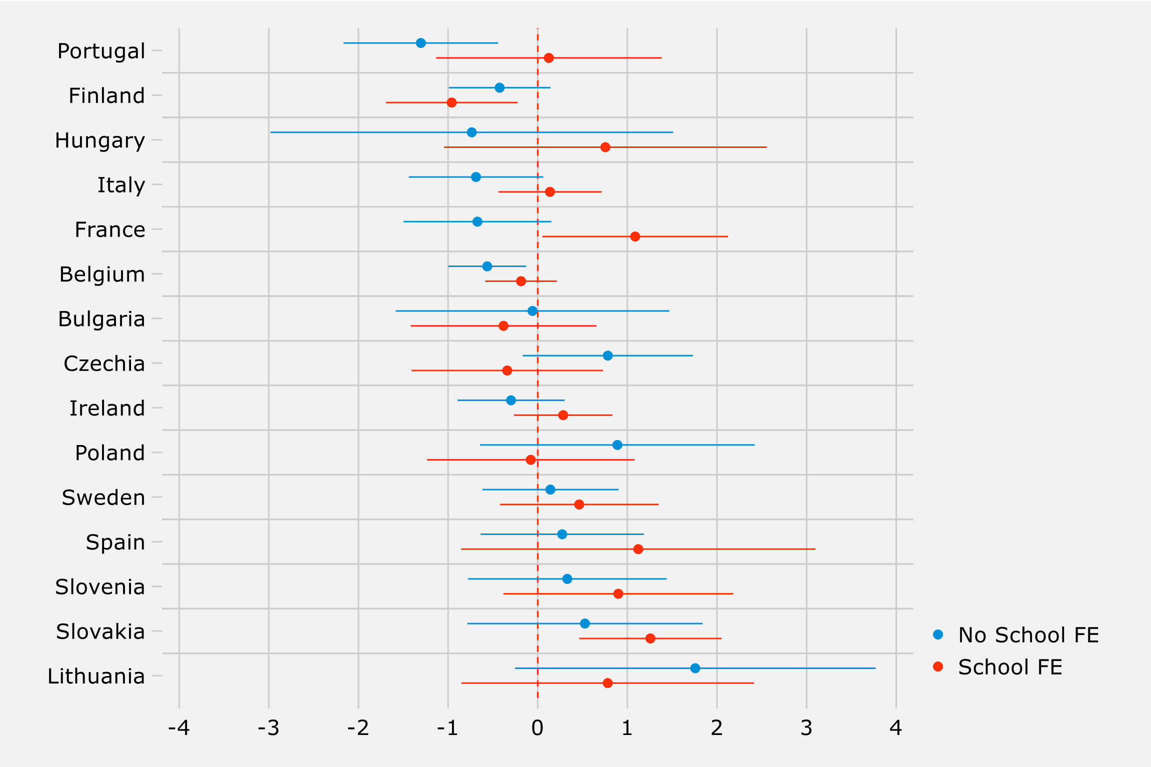
**Figure 3**. Immigrant peer effects. OLS and fixed effects estimated parameters

a) Reading Scores




[16] Full regression results are presented in the appendix.

b) Mathematics Scores




Let us first focus on Panel a) reporting results for PIRLS data on reading scores. The parameters show cross-country heterogeneity. They vary from negative 1.5 for Bulgaria and Portugal to positive 2 for Latvia and Spain. These results though, suggest a very limited effect - either positive or negative - of immigrant pupils on natives. In about half of the countries considered the effect is negative, but only in few of them it is also statistically significant. Portugal is the country where Immigrants peer-effect is the most negative and even here, increasing the percentage of immigrant pupils by one percentage point would decrease the reading score of a Portuguese born child by about 1.5 points - less than 2 hundreds of a standard deviation.

The Figure also shows cross-country variation in the effect of controlling for endogenous school sorting. This can be understood by comparing the point estimates obtained via OLS with the one obtained via the FE model. In many cases the differences between the two estimates are negligible, but for some cases such as Bulgaria, Czechia, Sweden, Spain and to some extent Italy, less so. It is also surprising to note how controlling for endogenous sorting sometimes *worsens* the negative spillover of students with a migrant background on native students or attenuates the positive impact for those countries where positive spillovers are found.

The small differences between the two set of estimators would seem to suggest a very limited sorting in European schools and this limited sorting would seem to counter the expected direction. Intuitively, we would expect that native sorting to operate in the opposite direction: highly educated or concerned parents take their children out of schools with a high concentration of students with a migrant background so that the native pupils left in those schools are the less advantaged ones. The estimates suggest that the opposite occurs. It needs to be noted though that the difference in the estimated OLS and FE coefficients are never statistically significant.

Turning the attention to mathematics scores in Panel b) we observe the same cross-country variability. The main feature of this set of results worth stressing is that while most spillovers are negative if estimated by OLS, they often become positive when selective sorting is accounted for in the fixed-effect model. In this case, selection affects the results in the expected direction in most cases. Only for Finland, Czechia, Poland, Lithuania and to some extent Bulgaria, the fixed effects estimates are more negative than the OLS ones. In general, most immigrant spillovers in mathematics scores are positive if estimated by FE even though they are often not statistically significant at the 5% level. Nonetheless, as for reading scores, both positive and negative spillovers are of very small magnitudes also in the case of mathematics scores. Looking at the FE estimates, we can see that the biggest peer effect is found in Slovakia where increasing the immigrant share in the class by 1 p.p. would increase the mathematics scores of natives by 1.2 points.

## 4.2 Including students with parents of a migrant background

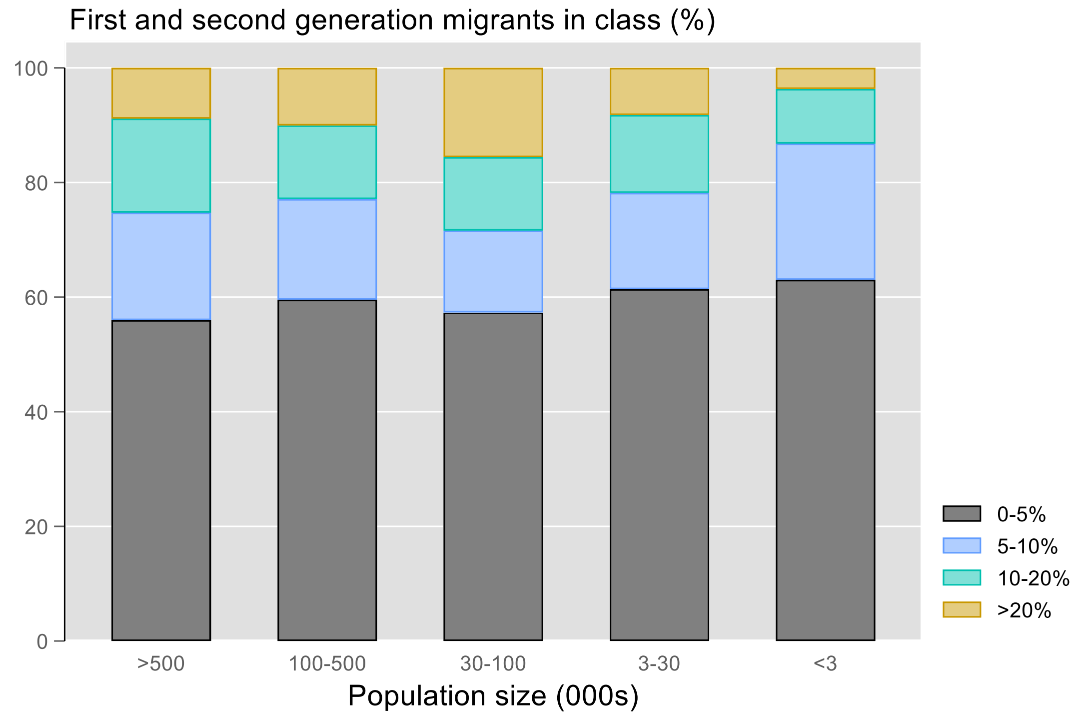
So far, I have defined as students with a migrant background only those children born in a country different from that of residence. These type of immigrants are here defined as students with a migrant background[17]. In this section, a different definition for immigrant is adopted. Along with students with a migrant background, also students born in the country, but whose parents are born outside of the country are defined as immigrants[18]. In the remainder of the paper, this second group is referred to as students with parents of a migrant background. In other words, in this section, both children with a migrant background and children with foreign-born parents are considered immigrants. This section tests whether this different definition changes the benchmark results.

Students with parents of a migrant background are usually more numerous than students with a migrant background and their educational achievements often retrace those of students with a migrant background (Dustmann et al., 2012). Even though they are born in the country of residence, they might require the same extra teaching resources and create the same disruptive learning environment as students with a migrant background, especially at very young age depending on their family environment. For example, if the language spoken regularly at home is that of origin of the parents, they might enter primary education with a more restricted vocabulary than their native peers and this may have negative repercussion on their learning and that of their peer via negative spillovers. It is therefore important to consider whether their presence might negatively affect the educational achievement of natives.

Unfortunately, PIRLS data do not include information on parents' country of birth; therefore, I necessarily restrict my analysis to TIMSS data. As spoken language is conceivably more relevant for reading performance, estimates based solely on math learning might offer a lower bound for immigrants peer effects on reading scores. In my sample, there are 2,393 children with parents of a migrant background in total.

Figure 4 shows the new class concentration of students with parents of a migrant background by geographical area. It plots the share of classes within each of the five geographical area that I have defined earlier, where migrants are less than 5%, 5 to 10%, 10 to 20% and more than 20%. Obviously, compared to Figure 1 the share of classes with more than 5% migrants increases and those with less than 5% decreases. Also adopting this new definition, students with parents of a migrant background tend to be more numerous in big cities and fewer as the area becomes more rural.

**Figure 4**. First and second-generation pupils' class concentration by geographic area

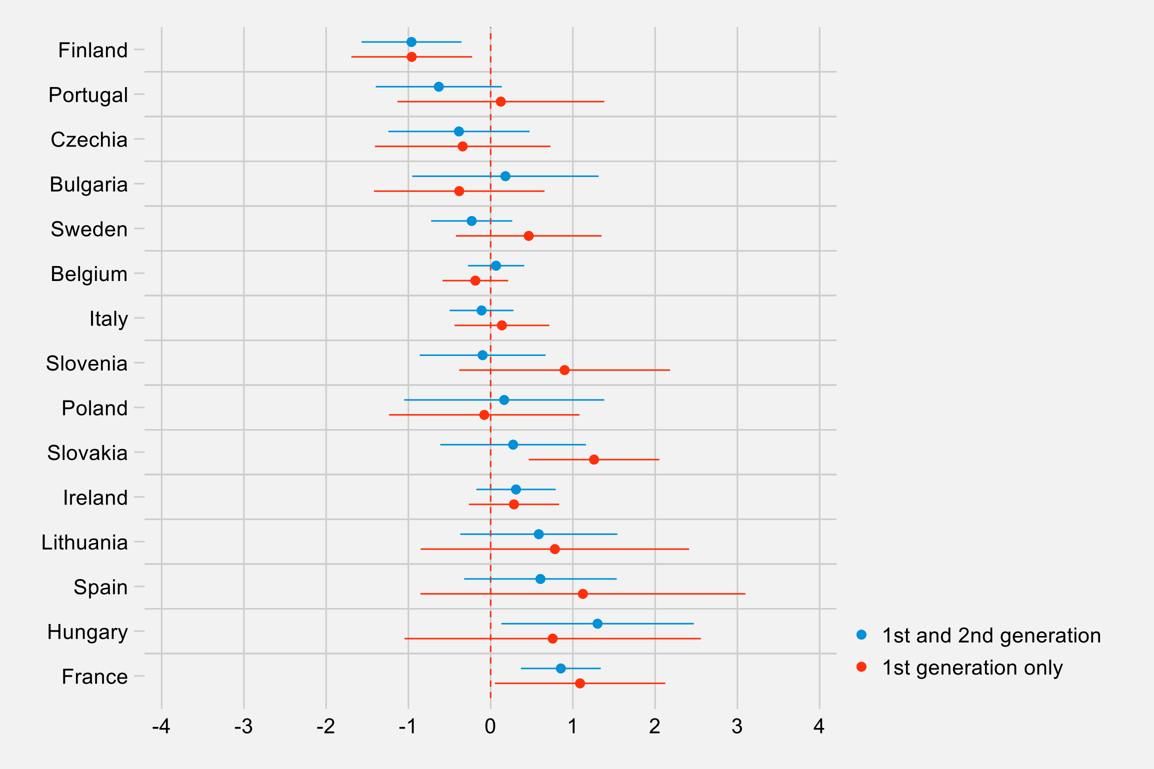



I can now turn to the estimation of peer effects where immigrant is now defined as first and second generation. The estimates are graphically presented in Figure 5. As for the baseline estimates in the previous section, I run separate regression for each country in my sample and include as additional regressors: a quadratic term in child age and gender, parents' educational level, teacher age, gender and years of experience and class size. I have also included school fixed effects so that, under my usual assumptions, the parameters can be interpreted causally. For sake of comparison I have added to the graph (in red) the parameters estimated in the baseline specification where the more restrictive definition of immigrant pupil was adopted. As for the graph, I have added the 95% confidence intervals to the point estimate and a vertical line at zero. If the confidence interval crosses the vertical dashed line, the parameter is not different from zero at the 95% confidence level.

The graph clearly shows that even including second-generation children within the migrants instead of the native group, in most cases, peer effects for math score are weak to absent. Only for Finland, I estimate a negative and statistically significant effect, but the effect is small: a 1 p.p. increase in class concentration diminishes math scores by 1 point. The only two other cases where the parameter is statistically significant are France and Hungary where I estimate a *positive* immigrant peer effect.

The graph also allows us to compare the difference in immigrant spillover if second generation children are defined as immigrants or natives. In most cases, differences are small. Only for Slovakia, a previously positive and significant effect becomes now positive, but smaller and not significant, while for Hungary a positive, but not significant parameter becomes now positive *and* significant

**Figure 5**. Immigrant peer effects including second-generation pupils – TIMSS data.




## 4.3 Urban vs. rural schools is there a difference?

This section analyses whether a urban/rural gap exists in the ability of schools to integrate students with a migrant background. Given the attention dedicated in the literature to rural learning gap on one the hand and the integration of students with a migrant background on the other, it is surprising how the two strands of literature have never been bridged. This omission is all the more surprising when one starts examining the specific peculiarities of education in rural areas and how those characteristics could interplay with the specific needs of students with a migrant background. Education in rural schools is different from the urban context and these differences might reasonably play against the integration of students with a migrant background. The most recent literature on immigrants' spillovers is discussed in Section 2; this section provides a discussion for the available evidence in rural/urban educational gaps and how the peculiar characteristics of students with a migrant background might affect them in rural schools.

There are at least two broad reasons why rural schools might be less effective in integrating students with a migrant background. The first has to do with differences in the characteristics of immigrant populations between the two areas; the second, with differences in the resources that institutions located in the two areas can dedicate to their students, both in total and on a per-capita basis.

Thanks to the attention dedicated to the measurement and the explanation of urban/rural divide in learning outcomes, we now have robust evidence that rural students, irrespective of their origin, are at a disadvantage when compared to students based in cities. In one of the most comprehensive study of educational outcomes in rural areas to date, Echazarra and Radinger (2019) analyze cross-country evidence from PISA survey and show that across OECD countries students in city schools outperform their rural counterparts. The learning gap is sizeable and amounts to one additional year of schooling in science for students of the same age. It needs to be said though that this average cross-country gap masks a lot of between-country heterogeneity. For example, they find an *urban* gap in Belgium, UK, and the US, while they find no gap for, among others, Germany and Spain.

Different family backgrounds between the two areas play a major role in explaining the existing rural gap. For example, Byun et al. (2012) for the US and Echazarra and Radinger (2019) for OECD countries find that if rural students were endowed with parents of equal socio-demographic standing as their urban counterpart, the rural gap would disappear or even reverse. In this context, the main socio-demographic driver is parental education which is, on average, lower in rural than urban communities. Lower educated parents are usually less involved in school matters and tend to understate the importance of education for future labour market success.

Another major driver of the urban/rural divide is the learning environment to which students are exposed. These environments generally differ between rural and urban schools and these differences often contribute to the creation and preservation of the gap. The two key differences are the geographic isolation and small size of rural schools. These two attributes diminish the chances that rural school attract high-quality teachers and adequate infrastructural funds.

Regarding teacher qualities, previous studies have found that teachers in urban schools are usually younger and more experienced (Echazarra and Radinger, 2019), but these differences are modest. More evident are differences in specific competence areas, particularly in science, where rural schools are asked to cope with frequent shortages of qualified teaching staff (Barter, 2008; Monk, 2007). Furthermore, even when quality teachers are employed, they might not be ready to teach in the specific rural context since teacher training is often designed with urban schools in mind (Ares Abalde, 2014) and they might be asked to teach a variety of subjects outside of their area of expertise (Barter, 2008). Funding is a particularly delicate issue for rural schools. Their small size makes the per capita expenditures higher and year on year costs projections much less predictable. Resource gaps are particularly evident for science teaching and for extra support activities geared towards special needs students.

Some of the identified fragilities of rural schools are particularly relevant for the integration of migrant students. In particular, given the more limited resources that they have access to, rural teachers are less likely to organize in-house development activities such as specific training in multicultural and multilingual teaching (Echazarra and Radinger, 2019) and are less aware of how to work with underrepresented and marginalized people (Biddle et al., 2018; Jorgensen et al., 2010).

PIRLS and TIMSS data allow to look directly at differences between urban and rural schools both in terms of general achievements and in terms of the spillover effects of students with a migrant background. The two surveys, in fact, contain questions that can be used to distinguish urban and rural schools. These questions, part of the school questionnaire, are answered by principals and teachers and among others, ask how many people live in the city (or area) where the school is located[19] and how the school's surrounding area could be defined in terms of population density[20]. I exploit the first of these two questions for the construction of my urban/rural indicator. More specifically, I define as urban those schools located in areas or cities whose population is above 30,000 and as rural the schools below this threshold. Applying this definition to my data, I classify 186 schools and 31,253 students as rural and 203 schools and 29,375 students as urban.

Table 4, using PIRLS data[21], displays some relevant descriptive statistics for urban and rural schools, students and teachers and a t-test for the differences. This table pools together students from all countries considered mainly for statistical reasons. Therefore, the numbers displayed need to be interpreted as cross-country averages.

The Table highlights meaningful differences between the two groups in a number of dimensions. Pupils in urban schools achieve higher reading scores, their parents are more highly educated, their teachers are almost half year more experienced and slightly older and they are thought in larger classes. They are also 0.68 percentage points more likely to have a migrant background than their rural counterparts. All these differences are statistically significant. More importantly, Table 4 shows significant differences between urban and rural schools in learning outcomes highlighting the existence of a rural gap for both mathematics and reading scores at cross-country level.

[19] For this question, seven possible categories were listed: More than 500,000; 100,001-500,000; 50,001-100,000; 30,001-50,000; 15,001-30,000; 3,001-15,000 and less than 3,000.

[20] The five available options for this question are: urban (densely populated; Suburban; Medium size city or large town; Small town or village and Remote or rural.

[21] TIMSS data, available on request, show similar results.

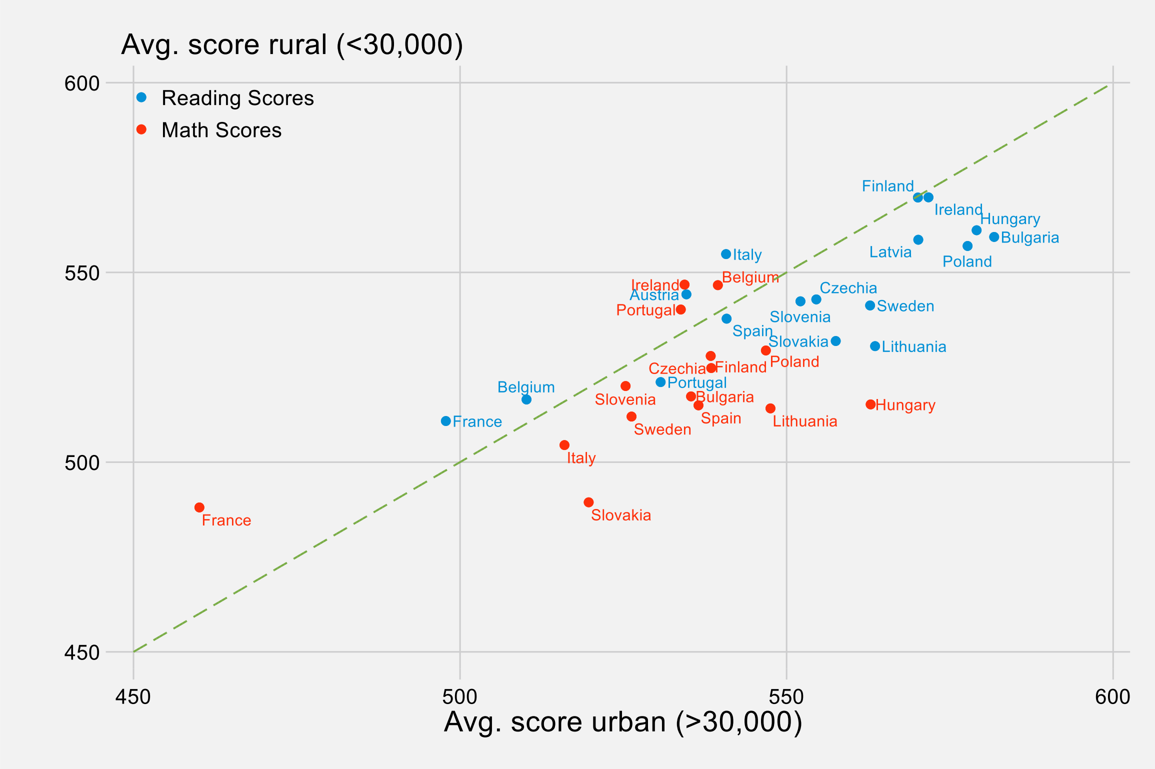
**Table 4.** Rural vs. Urban differences in observable characteristics – PIRLS data.

| | Rural | Urban | Diff. |
|---|---|---|---|
| Female | 0.498 | 0.499 | -0.002 |
| | | | (0.005) |
| (log)reading score | 6.280 | 6.319 | -0.038*** |
| | | | (0.001) |
| % First gen. migr. | 3.613 | 4.292 | -0.679*** |
| | | | (0.055) |
| Highest edu. father | 2.582 | 2.812 | -0.230*** |
| | | | (0.009) |
| Highest edu. mother | 2.739 | 2.989 | -0.251*** |
| | | | (0.009) |
| Teacher exp. (yrs.) | 19.442 | 19.879 | -0.437*** |
| | | | (0.102) |
| Teacher age (cat) | 3.859 | 3.999 | -0.140*** |
| | | | (0.010) |
| Class size | 22.186 | 23.759 | -1.573*** |
| | | | (0.038) |

Note: Standard errors in parentheses. PIRLS data.

In Figure 6 I go a step further and present the observed differences in the two scores disaggregated by country. The Figure plots the average score for urban and rural schools for all the countries considered where the blue dots represent reading scores and the red the math scores. For ease of interpretation, I have added a 45-degree line to the graph. A dot above the 45-degree line indicates a higher score in rural schools and a dot below the line the opposite. This graph makes at least three things clear: there is a positive correlation between the two scores, urban students outperform rural ones in most cases, but in most cases, the difference is small. France is one of the few cases where rural students outperform urban pupils both in math (considerably so) and reading. The other dots above the 45 degree line are only marginally so. It is also worth noting how the rural gap is wide in Slovakia (math), Lithuania (for both subjects) and Hungary (math).

**Figure 6**. Rural vs. Urban schools tests scores.




So far, only the rural/urban gap has been analysed, without tackling the issue of how the specific rural schools' characteristics might influence students with a migrant background. What the numbers presented, together with a reading of the literature, suggest, is that rural schools and teachers should be less apt to welcome special needs students such as this particular group; the only element working in their favour is the average smaller class size.

By including a series of dummy variables for four categories of urban density (in thousands): 100-500; 30-100; 3-30 and less than 3[22] and by interacting these dummy variables with the percentage of immigrant students in the class this paper provide a direct analysis of how schools in different geographical area react to an increase of students with a migrant background.

It needs to be noted though that since the geographical area characteristic does not vary within schools (e.g., all classes within a school will be located in the same area with the same population density), school fixed effects cannot be used for identification. Therefore, the estimates presented in this section necessarily rely on simple OLS estimator and leave potential school sorting unaccounted for. Given the results of my benchmark analysis presented in Section 4.1, this does not seem to be a crucial concern.

The results of the analysis are presented in Table 5. The first column reports the results for reading scores while the second column for math scores. Besides the variables shown in the Table, both models include a quadratic term in age, parental level of education, students and teachers gender, teacher experience and age, class size and country fixed-effects. Regressions are also weighted by country size.

[22] Together with cities above 500,000, the first two categories are what I have referred to as urban areas and the last three are the rural areas.

**Table 5.** Regression results of urban/rural gap.

| | Reading Scores | Math Scores |
|---|---|---|
| % First gen. migr. | -0.961* | 0.021 |
| | (0.002) | (0.972) |
| 100-500 | 2.565 | -8.341 |
| | (0.476) | (0.070) |
| 30-100 | -5.700 | -8.718* |
| | (0.095) | (0.037) |
| 3-30 | -2.973 | -11.610* |
| | (0.319) | (0.004) |
| <3 | 1.069 | -10.950 |
| | (0.821) | (0.083) |
| % First gen. migr. x | | |
| 100-500 | 0.371 | 0.012 |
| | (0.408) | (0.989) |
| 30-100 | 0.697 | -0.952 |
| | (0.405) | (0.169) |
| 3-30 | 0.728 | 0.150 |
| | (0.051) | (0.825) |
| <3 | -0.051 | -0.288 |
| | (0.945) | (0.745) |
| N | 45,885 | 40,935 |

Note: p-values in parentheses. * and + indicate significance levels at the 5 and 1% respectively. All regressions include controls for quadratic in age, pupil and teacher gender and their interaction, parents' highest educational achievement, teacher's age and work experience, class size and country fixed-effects. p-values clustered at school level.

Let us first look at the parameter describing students with a migrant background spillover effects for reading scores. As for the benchmark model, the spillover effect is captured by the percentage of students with a migrant background in class. This parameter is negative and statistically significant at the 5% level and it says that increasing the class share of immigrants' children by 1 p.p. would reduce natives' reading score by 0.96 points.

Let us now turn our attention to the urban/rural learning gap. This is captured by the four dummies for population density in the school area. The excluded category here is large cities above 500,000 inhabitants; therefore, the estimated parameters need to be interpreted as differences between reading scores for pupils in large cities and the other areas keeping the included characteristics constant. These estimates are too imprecise to draw any meaningful conclusion.

To examine whether rural schools are less able to integrate students with a migrant background the geographical dummies are interacted with the percentage of students with a migrant background in class. The interaction terms are presented in Table 5 and graphically in Figure 7. The coefficients in Table 5 are to be interpreted as the difference between schools in cities above 500,000 inhabitants and schools in the other areas in the effect of increasing the share of students with a migrant background in the classroom by 1 p.p. on reading scores. These differences are almost all positive pointing towards more negative spillover effects for large cities, but they are never statistically significant.

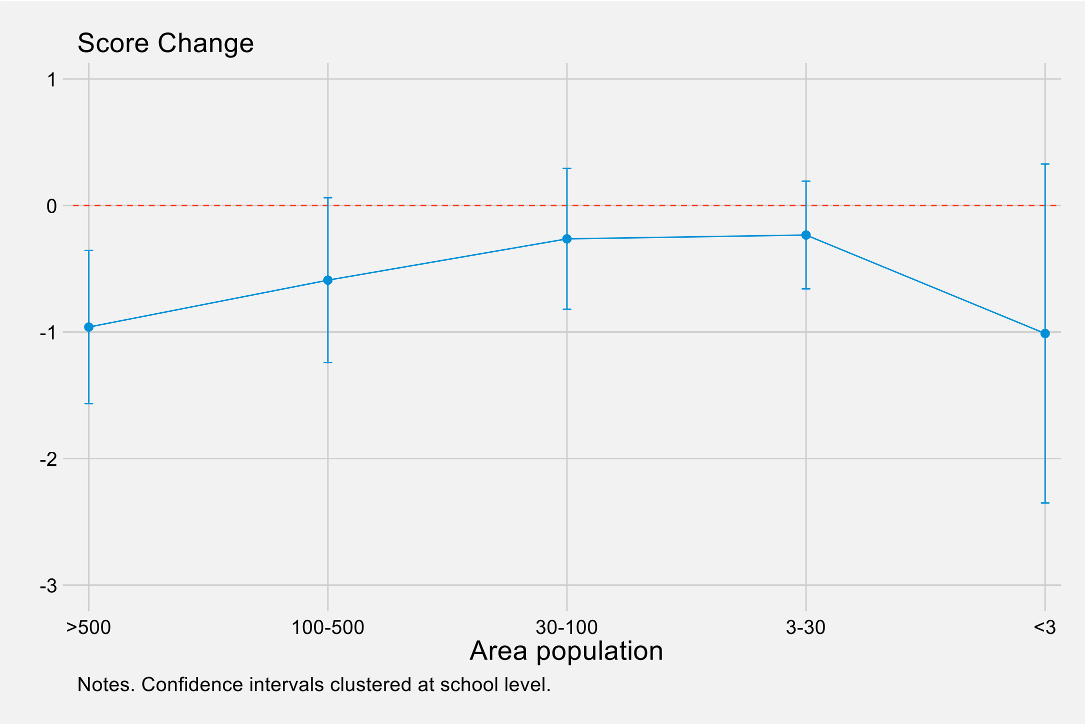
Figure 7 presents the same results, but in absolute terms instead of differences with big cities. From this figure, we note that students in rural schools are the most negatively affected from an increase in students with a migrant background population in class, but the magnitude of this effect is small and statistically significant only at confidence levels below 5%. For all other geographical areas, increasing the students with a migrant background population in the class has next to no effect on native students reading scores.

Looking at math scores in columns two, three things should catch our attention. First, the main spillover effect is now small and insignificant. Second, for math scores there is some evidence of a towns and small cities gap. Students in big cities outperform students in schools in all other geographical areas in math, but for small villages this difference is not significant. Third, as for reading scores, there is no geographical effect in spillover effects; they are small everywhere and very imprecisely estimated.

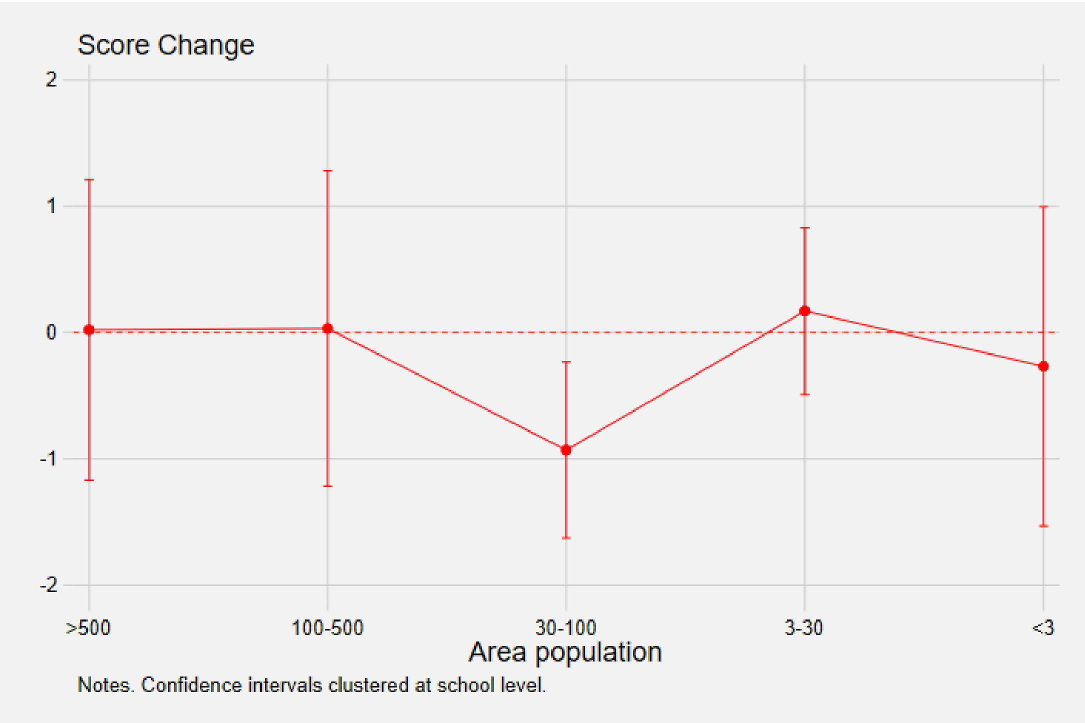
In conclusion, this analysis does not find evidence of a rural gap in reading abilities, while a *town* gap exists in math abilities, but there is no gap when it comes to students with a migrant background spillover on native students learning.

**Figure 7**. Effect of immigrant pupils' class share on natives' scores by area pop. Density

a) Reading scores




b) Mathematics scores



## 4.4 Non-linear spillover effects

The estimated spillover effects presented so far are obtained assuming that these effects are linear. That has to say that by construction my model has imposed that adding one student with a migrant background to a classroom where she is the only non-native will impact native students' learning in the same way as if that same student with a migrant background was added to a class where a large proportion of students is already of a migrant background. This assumption - called linear-in-means - might be too restrictive as some recent literature on peer effects suggests (Imberman et al., 2012; Sacerdote, 2011). Understanding what type of effect, linear-in-means or non-linear, is more appropriate has crucial and direct implications for school administrators and policy makers given that the two specifications imply very distinct mechanisms of social interaction translating in distinct optimal rules of class-formation.

A linear-in-means model posits that increasing the share of students with a migrant background always decreases native students' scores. This is the "bad apple model" of social interaction discussed by Lazear (2001). In this model one disruptive student is enough to exert negative peer effects on her schoolmates[23]. Alternatively, one could think that when disruptive students are few, the other students might easily integrate them. If this was the case, when students with a migrant background become prevalent, native students could reject them as their integration becomes more costly for them. In this case, the likelihood that students with a migrant background become disruptive is increasing in their share. This *integration model* predicts increasingly negative coefficients as the non-native share of immigrants in the classroom increases. Supportive evidence for this mechanism can be provided by allowing for non-linearities in the concentration of students with a migrant background in class.

In the context of immigrants' peer effects, some papers have found evidence of non-linearities. For example, Frattini and Meschi (2019), in a sample of Italian high school students, find non-linear negative effects, but only for math learning and only when immigrants' concentration in class exceeds 20% of the total. Another example of non-linear effects is found in Tonello (2016), again, in a sample of Italian students[24].

I will test for non-linearities in two ways: a) by including a quadratic term for the class share of students with a migrant background; b) by splitting the distribution of the proportion of students with a migrant background in class in quintiles and include these dummies in the regression model.

Table 6 presents the results for these tests for reading scores - column 1 and 2 - and math scores - columns 3 and 4. Both specifications confirm the lack of negative peer effect of students with a migrant background for both subjects and further add that this lack of effects remains even at relatively high share of students with a migrant background in class.

[23] Disruption can, but needs not be intended as 'bad' behaviour. Disruption, here, needs to be interpreted as anything that might slow down teaching activities such as need for additional help (Tonello, 2016).

[24] His econometric model does not allow for the identification of a specific threshold after which concentration becomes detrimental.

**Table 6.** Non-linear immigrant peers effect.

| | Reading scores | | Math scores | |
|---|---|---|---|---|
| | Quadratic | Quintiles | Quadratic | Quintiles |
| % First gen. migr. | -0.273 | | 0.173 | |
| | (0.300) | | (0.594) | |
| % Migrants sqrd. | 0.00313 | | -0.0107 | |
| | (0.779) | | (0.221) | |
| Quintiles: | | | | |
| 1st | | -0.380 | | 1.523 |
| | | (0.873) | | (0.487) |
| 2nd | | -1.372 | | -1.667 |
| | | (0.393) | | (0.563) |
| 3rd | | -3.391 | | 2.443 |
| | | (0.138) | | (0.465) |
| 4th | | -1.767 | | 1.481 |
| | | (0.414) | | (0.660) |
| 5th | | -3.786 | | -2.403 |
| | | (0.202) | | (0.518) |
| N | 46,783 | 46,783 | 41,857 | 41,857 |

Note: p-values in parentheses. * and + indicate significance levels at the 5 and 1% respectively. All regressions include controls for quadratic in age, pupil and teacher gender and their interaction, parents' highest educational achievement, teacher's age and work experience and class size. P-values clustered at country level.

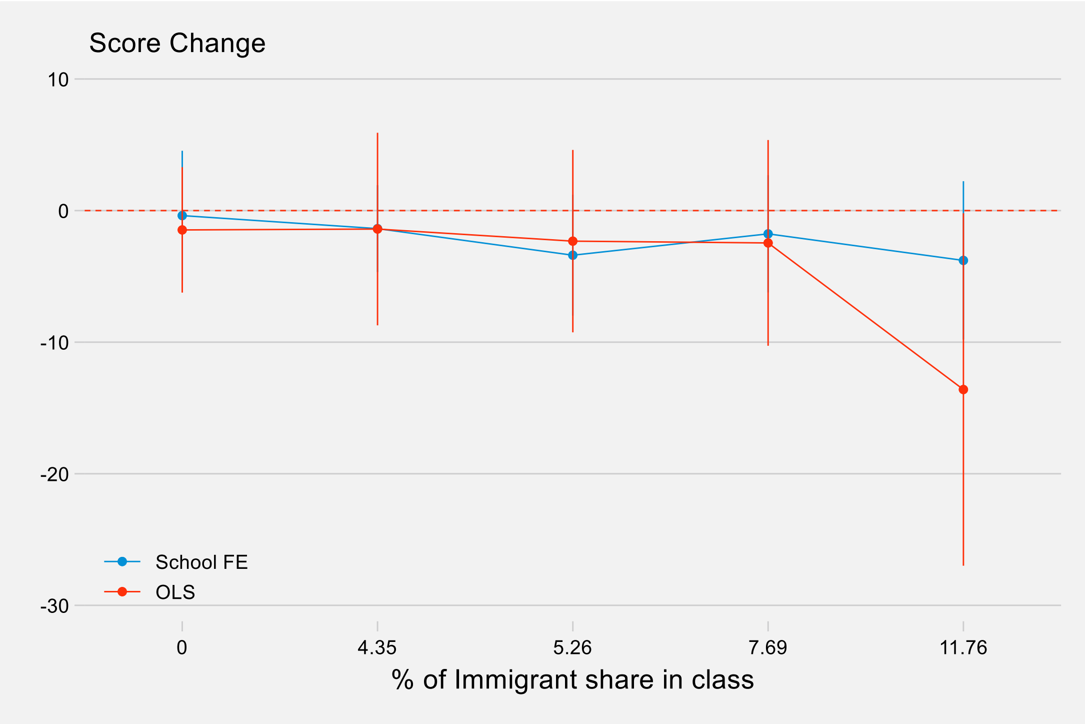
The parameters for the quintiles are presented graphically in Figure 8 where the lack of non-linear effect is immediately evident. This graph also plots the same non-linear effect, but estimated with a simple OLS estimator for sake of comparison. The comparisons of the OLS and fixed effect estimator is particularly interesting for reading scores where we see that if immigrant peer effect were estimated with by OLS, a negative and significant peer effects for the last quintile would be estimated.

This section shows how the lack of immigrant peer effects estimated in the benchmark linear-in-means model is unaffected by the specification of the peer effect parameters. This result partially echoes Frattini and Meschi (2019) who also fail to find non-linear peer effects for reading scores, but do find negative ones at high level of concentration (i.e. above 20%) for math score.

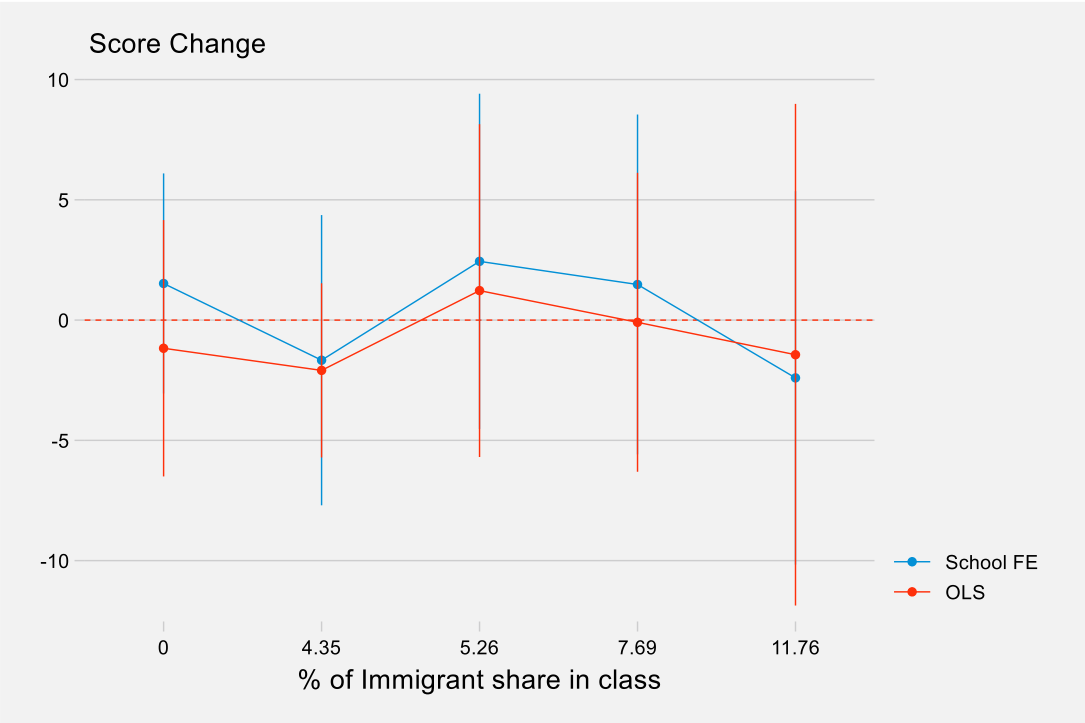
It needs to be said though that in this sample the within class immigrant share does not vary widely. The distribution is heavily skewed to the left with many zeroes, a mean value of the share of immigrant students per class of 8.79% for the non-zero classes and the 75th percentile of 10.53%. With this dataset, I cannot exclude that at higher concentrations, negative spillovers might start to emerge.

**Figure 8**. Native test score change at different levels of immigrant pupils' class concentration

a) Reading scores




b) Mathematics score



# 5 Conclusions

Immigration is a pressing concern for policy makers throughout Europe. For some Member States it is a phenomenon with a long history while for others is a new challenge. The waves of immigrants that have arrived in Europe recently were mostly consisting of young workers. Simple demographic considerations suggest that as this new waves of immigrants become more settled and start creating families, the question of their children scholastic performance and their influence on the children of natives will likely become more and more salient in the mind of parents and, as a consequence, central in the public debate. It is therefore necessary to gain an understanding of where European education and training system stand in terms of integration of children of an immigrant background. Unfortunately, evidence from Europe is still quite scarce especially when compared to the vast literature on immigrants' impacts on European labour markets. Even scarcer are systematic and comparable cross-national or cross-European studies.

The analysis presented in this paper has exploited a very comprehensive international database analysing reading and math skills for a sample of children in primary school for 19 Member States to investigate the effect that students with a migrant background have on the learning of native students.

The findings are in line with the most recent and robust economic research of immigrant pupils' spillover in different European contexts: the effects of immigrant children on native children learning especially in reading abilities are negative. However, these negative effects disappear, in most countries, once personal, teacher and school characteristics as well as selective sorting into schools is controlled for. This result is maintained even when spillovers are allowed to operate non-linearly. Furthermore, the cross-European focus allows documenting some variation in the ability of national educational systems to integrate immigrant children. Nevertheless, even the most negative estimates found for Bulgaria and Portugal in reading and Portugal and Finland in math, are small in magnitudes. Lastly, the question of the existence of a rural gap in immigrant children integration is tackled, finding none. To the extent that within school comparisons capture unobserved sorting these results can be interpreted causally.

It needs to be stressed thought that even though some of my estimates are precisely estimated at zero, for others, the lack of relationship is due to lack of precision. It would be advisable to repeat this type of analysis on larger samples of classes and schools and check whether the essential lack of negative spillover effects would survive in a larger dataset. It would also be advisable to perform this type of analysis on data that allow distinguishing between children of Third Country Nationals and those of mobile EU citizens. These two group of pupils possibly display different needs and, in the context of the EU and its integration policies, might require distinct type of interventions.

The heterogeneity between MSs in the effectiveness of integration that this analysis displays should motivate further research into understanding what makes some educational systems more suited than others to integrate disadvantaged students. Such analysis could start by looking at how teacher characteristics help explain the different performances.

Likewise, it is important to recognize that test scores measure only part of the complex dynamics that influence personal success that are determined in school. My analysis focused on those measures and has found negligible immigrants peer effects, in line with most previous literature, but it is silent on other, non-cognitive skills influenced by the learning environment. These type of skills are equally or possibly even more crucial in influencing future economic outcomes as some recent literature highlights (Jackson, 2018). These aspects have been, so far, sorely neglected. A promising avenue of future enquiry is to study whether peer effects might affect those skills.

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## List of abbreviations and definitions

| | |
|---|---|
| EU | European Union |
| FE | Fixed-effects |
| IAE | International Association for the Evaluation of Educational Achievement |
| MS | Member State |
| OECD | Organization for Economic Co-operation and Development |
| OLS | Ordinary Least Squares |
| PIRLS | Progress in International Reading Literacy Study |
| PISA | Programme for International Student Assessment |
| TIMSS | Trends in International Mathematics and Science Study |

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## Annexes

**Annex 1. Fixed effect full estimates baseline model – PIRLS data.**

| | Austria | Belgium | Bulgaria | Czechia | Finland | France | Hungary | Ireland | Italy |
|---|---|---|---|---|---|---|---|---|---|
| % First gen. migr. | 0.175 | -0.979* | -1.634* | -0.990* | -0.580 | -0.479 | 1.047 | -0.891+ | 0.441 |
| | (0.44) | (-5.09) | (-3.22) | (-2.87) | (-1.62) | (-1.09) | (1.73) | (-2.47) | (0.89) |
| Age | 630.9* | 172.4* | 73.48 | 250.7 | 838.0* | 373.0* | 250.8+ | 338.5 | 114.2 |
| | (7.98) | (3.83) | (0.58) | (1.42) | (6.44) | (3.74) | (2.27) | (1.96) | (0.76) |
| $Age^2$ | -30.27* | -8.764* | -2.932 | -12.37 | -38.17* | -19.67* | -12.19+ | -15.91 | -4.823 |
| | (-7.91) | (-3.95) | (-0.50) | (-1.45) | (-6.37) | (-3.87) | (-2.36) | (-1.93) | (-0.63) |
| Highest edu. father | 10.68* | 10.41* | 7.138* | 13.39* | 8.283* | 10.88* | 9.007* | 12.98* | 11.38* |
| | (7.13) | (12.04) | (4.26) | (10.07) | (6.42) | (7.12) | (5.62) | (7.13) | (4.59) |
| Highest edu. mother | 10.40* | 11.07* | 15.69* | 10.29* | 11.91* | 9.547* | 10.60* | 9.294* | 11.54* |
| | (7.00) | (12.62) | (7.34) | (7.81) | (8.86) | (5.34) | (7.16) | (5.27) | (5.96) |
| Female teacher | 26.87* | 3.713 | 13.51 | 13.04 | -0.0784 | 1.319 | 6.298 | 6.153 | -6.530 |
| | (3.06) | (0.76) | (1.10) | (1.48) | (-0.02) | (0.19) | (0.70) | (0.95) | (-0.74) |
| Female | 27.90* | 9.924+ | -2.738 | -0.368 | 21.71* | 1.175 | 4.525 | 10.43 | 4.770 |
| | (3.32) | (2.51) | (-0.34) | (-0.04) | (5.88) | (0.22) | (0.27) | (1.42) | (0.33) |
| Female teac. x Female | -20.09+ | -0.312 | 15.85 | 7.099 | -0.0595 | 12.08+ | 3.896 | 0.795 | 6.921 |
| | (-2.30) | (-0.07) | (1.86) | (0.68) | (-0.01) | (1.98) | (0.23) | (0.10) | (0.47) |
| Teacher exp. (yrs.) | 0.534 | 0.555 | -0.213 | 0.525+ | -0.120 | -0.226 | 0.596* | -0.872 | -0.0505 |
| | (1.25) | (1.76) | (-0.65) | (2.28) | (-0.56) | (-0.46) | (2.68) | (-1.38) | (-0.12) |
| Teacher age: | | | | | | | | | |
| 25-29 | 23.42+ | -2.670 | | -7.726 | 10.16* | 7.904 | | 18.08* | |
| | (2.00) | (-0.36) | | (-1.34) | (2.90) | (0.91) | | (2.88) | |
| 30-39 | 22.56+ | -3.305 | -25.37+ | -3.110 | 15.25* | 26.09* | 24.47* | 20.48* | 37.69* |
| | (1.99) | (-0.44) | (-2.09) | (-0.48) | (2.64) | (3.14) | (2.90) | (2.84) | (6.69) |
| 40-49 | 29.08+ | -12.76 | 4.625 | -12.15 | 15.69+ | 23.95+ | 8.131 | 10.22 | 23.09+ |
| | (2.25) | (-1.43) | (0.48) | (-1.96) | (2.53) | (2.28) | (0.97) | (0.95) | (2.08) |
| 50-59 | 36.48+ | -15.01 | 10.76 | -21.05* | 21.07+ | 28.96 | 1.828 | 39.71 | 31.01+ |

| | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| | (2.30) | (-1.30) | (0.84) | (-2.88) | (2.58) | (1.83) | (0.21) | (1.95) | (2.18) |
| +60 | 30.62 | -65.64* | 9.779 | -21.23 | 19.89+ | 67.88* | 10.23 | | 36.66 |
| | (1.44) | (-4.14) | (0.61) | (-1.80) | (2.44) | (3.55) | (1.04) | | (1.98) |
| Class size | 1.366 | -0.553 | 1.305 | 1.117+ | 0.884 | -0.971 | 3.161* | 0.152 | 1.242+ |
| | (1.37) | (-1.49) | (1.01) | (2.21) | (0.75) | (-0.59) | (4.54) | (0.10) | (2.15) |
| N | 4,825 | 6,172 | 2,195 | 3,674 | 3,342 | 2,194 | 2,453 | 2,124 | 1,607 |
| | Latvia | Lithuania | Poland | Portugal | Slovakia | Slovenia | Spain | Sweden | |
| % First gen. migr. | 1.819* | 0.398 | -0.392 | -1.628* | 0.201 | -0.242 | 1.481* | 0.128 | |
| | (3.86) | (0.67) | (-0.79) | (-2.96) | (0.32) | (-0.66) | (3.83) | (0.36) | |
| Age | 233.0* | 69.51 | 189.7* | 278.1* | 294.7* | 388.2* | 591.5* | 221.3 | |
| | (3.56) | (0.75) | (3.62) | (4.60) | (3.43) | (3.83) | (3.45) | (0.69) | |
| $Age^2$ | -10.64* | -3.158 | -8.711* | -14.07* | -14.76* | -19.21* | -29.92* | -9.581 | |
| | (-3.56) | (-0.71) | (-3.58) | (-4.81) | (-3.60) | (-3.81) | (-3.47) | (-0.64) | |
| Highest edu. father | 7.540* | 13.17* | 10.14* | 5.454+ | 8.248* | 14.46* | 9.311* | 10.85* | |
| | (5.06) | (5.86) | (6.89) | (2.37) | (6.05) | (9.20) | (5.81) | (5.77) | |
| Highest edu. mother | 8.225* | 9.387* | 13.36* | 13.57* | 11.37* | 16.85* | 9.269* | 11.05* | |
| | (5.17) | (4.09) | (10.17) | (7.89) | (10.30) | (11.15) | (6.84) | (6.61) | |
| Female teacher | 11.66 | -18.68 | -2.124 | 10.42 | -7.459 | -7.087 | 1.519 | 10.42 | |
| | (1.11) | (-0.83) | (-0.28) | (1.03) | (-0.61) | (-0.60) | (0.33) | (1.53) | |
| Female | 60.58 | -29.46* | 17.95 | -4.666 | 2.735 | -2.737 | 0.914 | 17.16+ | |
| | (1.75) | (-6.84) | (1.01) | (-0.50) | (0.35) | (-0.22) | (0.18) | (2.16) | |
| Female teac. x Female | -43.93 | 38.53* | -0.168 | 4.362 | 5.458 | 20.95 | 5.656 | -2.921 | |
| | (-1.27) | (7.26) | (-0.01) | (0.44) | (0.68) | (1.66) | (0.90) | (-0.35) | |
| Teacher exp. (yrs.) | 0.212 | 0.598 | 1.657* | 1.635 | -0.512 | 0.565 | 0.727+ | -0.617+ | |
| | (0.62) | (1.33) | (3.25) | (1.68) | (-1.58) | (1.16) | (2.54) | (-2.20) | |
| Teacher age: | | | | | | | | | |
| 25-29 | 42.94+ | 6.217 | | | | -9.993 | -11.74* | -13.05 | |
| | (2.28) | (0.27) | | | | (-1.11) | (-5.85) | (-1.28) | |
| 30-39 | 61.29* | 19.17 | -5.152 | | -3.929 | -27.33* | -5.920 | 7.793 | |
| | (3.08) | (1.15) | (-1.09) | | (-0.43) | (-3.89) | (-0.81) | (0.63) | |

| | | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| 40-49 | 56.16* | 2.835 | -13.67 | -9.469 | 8.842 | -36.21* | -13.61 | -0.688 |
| | (2.92) | (0.20) | (-1.24) | (-1.10) | (0.88) | (-3.02) | (-1.59) | (-0.06) |
| 50-59 | 54.64* | -2.999 | -35.01+ | -26.11 | 4.897 | -44.02+ | -23.78+ | 4.909 |
| | (2.67) | (-0.16) | (-2.40) | (-1.62) | (0.43) | (-2.58) | (-2.57) | (0.36) |
| +60 | 54.88+ | -7.069 | -59.85* | -6.606 | 19.01 | -27.68 | -34.09+ | 15.61 |
| | (2.49) | (-0.32) | (-3.40) | (-0.28) | (1.32) | (-1.32) | (-2.22) | (0.90) |
| Class size | 0.765* | 2.508+ | 0.544 | 1.229 | -0.369 | 1.553 | 0.743* | 1.417 |
| | (3.45) | (2.58) | (1.15) | (1.13) | (-0.41) | (1.33) | (4.42) | (1.17) |
| N | 2,065 | 1,112 | 3,149 | 1,343 | 3,472 | 2,911 | 1,841 | 1,910 |

Note: p-values in parentheses. + and * indicate significance levels at the 5 and 1% respectively. P-values clustered at school level.

**Annex 2. Fixed effect full estimates baseline model – TIMSS data.**

| | Belgium | Bulgaria | Czechia | Finland | France | Hungary | Ireland | Italy | Lithuania |
|---|---|---|---|---|---|---|---|---|---|
| % First gen. migr. | -0.186 | -0.382 | -0.340 | -0.959+ | 1.087+ | 0.755 | 0.284 | 0.137 | 0.781 |
| | (-0.92) | (-0.73) | (-0.63) | (-2.58) | (2.07) | (0.83) | (1.03) | (0.47) | (0.95) |
| Age | 30.79 | 191.9* | 549.6* | 880.6* | 386.4+ | 365.4* | 441.3 | 715.7+ | -39.38 |
| | (0.56) | (3.22) | (4.03) | (4.63) | (2.54) | (5.26) | (1.95) | (2.49) | (-0.26) |
| Age$^2$ | -1.848 | -8.802* | -27.11* | -40.42* | -20.34* | -17.80* | -20.82 | -35.94+ | 1.789 |
| | (-0.68) | (-3.26) | (-4.15) | (-4.57) | (-2.67) | (-5.50) | (-1.91) | (-2.44) | (0.25) |
| Highest edu. father | 7.643* | 12.12* | 13.82* | 8.483* | 11.00* | 10.61* | 15.61* | 10.57* | 14.05* |
| | (6.30) | (7.88) | (9.30) | (6.05) | (4.69) | (5.53) | (7.66) | (3.96) | (5.37) |
| Highest edu. mother | 9.928* | 13.73* | 14.86* | 11.76* | 17.67* | 16.78* | 13.71* | 11.89* | 10.43* |
| | (7.82) | (8.99) | (10.35) | (7.17) | (7.79) | (11.73) | (6.36) | (4.77) | (3.83) |
| Female teacher | -0.123 | 4.175 | -0.829 | -6.658 | -7.150 | 23.90* | -4.202 | 16.50 | 0 |
| | (-0.03) | (0.36) | (-0.14) | (-1.89) | (-1.14) | (3.35) | (-0.54) | (1.49) | (.) |
| Female | -9.160+ | 5.680 | -3.761 | -1.408 | -24.18* | -12.54 | -4.768 | -18.62+ | 2.426 |
| | (-2.16) | (0.69) | (-0.54) | (-0.37) | (-3.15) | (-1.27) | (-0.49) | (-2.17) | (0.61) |
| Female teach. x Female | -0.869 | -5.701 | -8.385 | 7.617 | 15.04 | -5.747 | -2.241 | 2.239 | 0 |
| | (-0.18) | (-0.67) | (-1.17) | (1.86) | (1.93) | (-0.57) | (-0.22) | (0.24) | (.) |
| Teacher exp. (yrs.) | 0.707+ | 0.124 | 0.448+ | 0.826* | 0.703+ | 0.0633 | 0.282 | 0.284 | 0.756 |
| | (2.51) | (0.35) | (2.09) | (3.07) | (2.50) | (0.24) | (0.53) | (1.34) | (1.66) |
| Teacher age: | | | | | | | | | |

| | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| 25-29 | -5.721 | | -25.09+ | -1.580 | -12.78 | | 5.811 | | |
| | (-0.65) | | (-2.45) | (-0.12) | (-1.58) | | (0.63) | | |
| 30-39 | -11.53 | -9.266 | -18.15 | 3.678 | -20.14* | -9.234 | -8.395 | 4.356 | 13.31 |
| | (-1.23) | (-0.61) | (-1.57) | (0.33) | (-2.67) | (-0.89) | (-0.89) | (0.36) | (0.73) |
| 40-49 | -17.73 | -11.14 | -20.50 | -3.963 | -22.90* | -9.199 | 17.09 | 4.503 | 11.13 |
| | (-1.82) | (-0.78) | (-1.65) | (-0.34) | (-3.06) | (-0.85) | (1.75) | (0.39) | (0.68) |
| 50-59 | -25.22+ | -10.06 | -22.26 | -14.73 | -24.68+ | -12.62 | -29.99 | 5.269 | 1.464 |
| | (-2.11) | (-0.69) | (-1.63) | (-1.15) | (-2.53) | (-1.10) | (-1.70) | (0.44) | (0.08) |
| +60 | -12.00 | -17.77 | -41.27* | -14.20 | -20.06 | -17.39 | -3.943 | -0.836 | 1.218 |
| | (-0.68) | (-1.09) | (-2.83) | (-1.04) | (-1.53) | (-1.35) | (-0.28) | (-0.06) | (0.06) |
| Class size | -0.660 | -3.954+ | 2.499* | 2.110* | -1.265 | 1.517 | 0.406 | -1.165+ | 3.854* |
| | (-1.01) | (-2.23) | (2.70) | (2.95) | (-0.68) | (1.95) | (0.35) | (-2.07) | (4.03) |
| N | 2,432 | 3,112 | 4,461 | 3,836 | 1,947 | 4,320 | 1,681 | 2,253 | 1,105 |

| | Poland | Portugal | Slovakia | Slovenia | Spain | Sweden |
|---|---|---|---|---|---|---|
| % First gen. migr. | -0.0774 | 0.124 | 1.257* | 0.899 | 1.122 | 0.463 |
| | (-0.13) | (0.20) | (3.12) | (1.39) | (1.19) | (1.04) |
| Age | 360.7* | 477.3* | 92.59* | 1001.4* | 2851.6* | 206.1 |
| | (6.58) | (4.33) | (3.26) | (3.48) | (5.22) | (1.97) |
| $Age^2$ | -17.19* | -24.11* | -5.052* | -50.55* | -142.7* | -8.522 |
| | (-6.66) | (-4.51) | (-3.94) | (-3.46) | (-5.17) | (-1.79) |
| Highest edu. father | 12.69* | 10.24* | 9.763* | 7.180* | 12.33* | 9.247* |

| | | | | | | |
|---|---|---|---|---|---|---|
| | (8.86) | (3.84) | (7.14) | (3.13) | (3.89) | (4.69) |
| Highest edu. mother | 12.71* | 13.42* | 13.77* | 21.35* | 8.873+ | 10.50* |
| | (10.49) | (5.57) | (9.38) | (9.88) | (2.21) | (5.08) |
| Female teacher | 11.40* | 4.045 | 5.443 | 1.766 | 5.196 | 4.664 |
| | (2.81) | (0.56) | (0.69) | (0.12) | (0.82) | (0.79) |
| Female | 6.904 | -9.062 | -0.242 | -5.235 | -8.181 | -3.790 |
| | (1.01) | (-0.91) | (-0.03) | (-0.32) | (-0.81) | (-0.59) |
| Female teach. x Female | -11.26 | -5.031 | -7.550 | -3.873 | -12.03 | 6.507 |
| | (-1.64) | (-0.47) | (-0.95) | (-0.23) | (-1.12) | (1.04) |
| Teacher exp. (yrs.) | 0.338+ | 1.632 | -0.129 | 0.720 | 0.330 | -0.885* |
| | (2.33) | (1.81) | (-0.43) | (1.51) | (1.27) | (-3.28) |
| Teacher age: | | | | | | |
| 25-29 | 9.036* | | | | | -22.33+ |
| | (3.44) | | | | | (-2.04) |
| 30-39 | -2.653 | | -3.041 | 0.607 | 8.343 | -10.57 |
| | (-1.55) | | (-0.53) | (0.03) | (1.21) | (-1.24) |
| 40-49 | -4.338 | 0.273 | 9.041 | 0.648 | 3.199 | -11.18 |
| | (-1.79) | (0.03) | (1.13) | (0.03) | (0.39) | (-1.42) |
| 50-59 | -6.130 | -16.75 | 7.084 | -16.20 | -10.55 | 7.433 |
| | (-1.51) | (-0.87) | (0.73) | (-0.64) | (-1.06) | (1.24) |
| +60 | -10.47 | -32.91 | 4.516 | -8.924 | -11.49 | 15.79 |

| | | | | | | |
|---|---|---|---|---|---|---|
| | (-1.62) | (-1.41) | (0.36) | (-0.33) | (-1.16) | (1.79) |
| Class size | -0.177 | -0.247 | 0.126 | -0.458 | 7.429 | 2.467* |
| | (-0.82) | (-0.23) | (0.13) | (-0.69) | (2.03) | (2.90) |
| N | 6,884 | 1,028 | 4,969 | 1,339 | 471 | 1,759 |

Note: p-values in parentheses. + and * indicate significance levels at the 5 and 1% respectively. P-values clustered at school level.

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